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**DID THE US INFERTILITY HEALTH INSURANCE MANDATES
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Did the US infertility health insurance mandates affect the timing of first birth?

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Abstract

From 1977-2001, 15 US states mandated health insurance providers to offer coverage for infertility treatment. Although the majority of the past literature has studied impacts on older women who are likely to seek treatment, this paper proposes that the mandates may have had a wider impact on the US population. Specifically, it may have given an option for younger women to delay birth since these policies reduced the opportunity cost of having a child in the future. Results suggest a significant delay of 1-2 years in the time of first birth among highly educated white women.

JEL classification: I18; J13; J18

Keywords: Infertility; Insurance mandates; Fertility; Timing of birth

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1 Introduction

Currently 6 million women in the United States experience difficulties in conceiving a child even after a year of unprotected intercourse. The figures for the proportion of women who faced impaired fecundity increased from 8 percent in 1982 to 10 percent in 1995 (Chandra and Stephen, 1998). Whilst various infertility treatment options are available to assist these couples, they are often extremely costly. Some countries provide financial help, but in the U.S. patients often faced the full financial burden of the treatment.¹ Between 1977 and 2001, state-level legislation was introduced in 15 states which mandated health insurance providers to offer coverage for the fertility treatment cost.

Mosher and Bachrach (1996) suggest that the observed increase in the number of US women suffering from infertility problems is not caused by an increase in the rate of infertility, but rather due to more women postponing their fertility activities. This delay of motherhood is prominent among women with higher educational attainment as well as stronger labor market attachment (Rindfuss, Morgan, and Offutt, 1996). One possible reason for observing the delay particularly among highly educated women is the difficulties they face in balancing work and life. Phipps, Burton, and Lethbridge (2001) document that, on average, women have more job interruptions than men and 80 percent of these interruptions are related to motherhood. Since there is a substantial amount of evidence pointing out possible detrimental impacts of career interruptions on women's future wages (for example, see Eckstein and Wolpin, 1989; Altuğ and Miller, 1998; Korenman and Neumark, 1992), women with high career ambitions may be postponing births to advance in their workplaces.

If this is the case, the introductions of state-level infertility insurance mandates may have induced women to further delay giving birth, since the knowledge of accessible and affordable infertility treatment may have led women to focus on their careers for a longer period of their lives and postpone giving birth. Given that the majority of women

¹For example, the public system in Denmark offers up to three cycles of In Vitro Fertilization treatment for free.

who obtain treatment are older and highly educated white women who have stronger attachment than average to the labor market, such an argument is rather plausible (Bitler and Schmidt, 2007).

Previous literature on the US infertility insurance mandates has mainly highlighted the impact of mandates on the take up and the outcomes of treatment among an older group of women. This is a logical choice of the age group to study as women who need and thus access the treatment are often those who are above 35. In contrast, this paper investigates a potentially unintentional impact among younger women. In particular, this paper studies how the U.S. infertility insurance mandates affected the timing of first birth of young women by employing the framework of event history analysis. The impacts of mandates are identified using the difference-in-differences approach by comparing the timing of births for women residing in states with and without the mandates. The main data is taken from the 1980-1997 Panel Study of Income Dynamics (PSID) and the Current Population Survey (CPS).

The focus on the timing of first birth among younger women is appealing, since understanding how younger women's decisions to have a child alter in response to such financial incentives is an interesting question on its own. Moreover, although these women having the wider choice of birth timing is welfare improving, the increase in the age at first birth is likely to cause various negative health outcomes. For example, Menken, Trussell, and Larsen (1986) report that delay of birth from the age group 25-29 to 30-34 increases the proportion of infertile women from 9 to 15%. First births at older ages are also associated with health risks for both the mothers and the new born children (the American Society for Reproductive Medicine (ASRM), 2003) . The probability of down syndrome increases for birth after 30 and the probabilities of miscarriages and pregnancy complications also increase for older mothers. The delay would, therefore, be likely to increase the health care costs due not only to the higher demand for infertility treatment but also to the more intensive pre and post-natal care required. Lastly, several of the previous works on this topic use young women as a control group in order to

investigate the impact of mandates on older women. These papers typically assume that younger women are unaffected by the introduction of the infertility health insurance mandates. If, however, younger women are indeed delaying birth, the policy impacts would be overestimated.

The remainder of this paper is organized as follows. The next section looks at background information regarding the U.S. infertility treatment and the health insurance system as well as the structure of the state mandates. Section 3 describes the theoretical framework and 4 looks at the past literature. Section 5 and 6 each describes the identification strategy and the empirical specification. Section 7 presents the data used for the analysis. Section 8 and 9 describe the results and robustness analysis respectively. Section 10 concludes.

2 Background

2.1 Infertility treatment

The initial step taken by couples seeking treatment is the examination of both partners' reproductive organs. As a next step, the majority undergo several less invasive methods such as the use of fertility drugs which induce women to produce multiple eggs per ovulation. If the cause of infertility is clear, women proceed straight to surgery in order, for example, to unblock their fallopian tubes. Whilst most women successfully conceive a child without using more invasive methods, a small proportion of women proceed to receive treatment via the Assisted Reproductive Technologies (ART), which are any treatments that handle either sperms and eggs or both. Details of these treatments are summarized in Table 1.

Although two thirds of couples who seek treatment in the U.S. successfully conceive children, the success rates vary with the age of women. For example, the pregnancy rates of ART for women aged 29 is 44.9 percent but this figure drops to 37.6 percent for women aged 35 (the Centers for Disease Control and Prevention, 2005).

Infertility treatments are often very costly. Hormone therapies which are used to induce the releasing of an egg could cost between \$50 and \$5,000 per cycle whilst tubal surgery would cost between \$3,000 and \$10,000 (see Table 1). One of the most expensive treatments, In Vitro Fertilization (IVF) on average costs \$12,400 per cycle, although this treatment only accounts for approximately 5 percent of the U.S. infertility treatment (ASRM, 2009) . When these treatments are combined or used for repeated cycles, the financial burden for patients quickly becomes too heavy for them to continue the infertility treatment.

2.2 Structure of health insurance in the United States

High medical cost in the US is covered by various forms of health care insurance. The US health care insurance can be divided into public and private insurance. Public or federally funded programs are under the control of federal laws and currently cover approximately 27 percent or 85 million individuals that often face difficulties obtaining private insurance policies (DeNavas-Walt, Proctor, Smith, and the Bureau of the Census, 2008). Private insurance, however, is the relevant insurance for the purpose of this paper as the state-level mandates only affect those that are insured privately.

Private insurance policies are either purchased individually or through employers under group purchasing agreements. Whilst only 12 percent of individuals purchase their own insurance, the majority obtain their coverage through their employers (DeNavas-Walt et al., 2008). The importance of employer sponsored insurance in the U.S. is evident from the sheer number of individuals that are covered by their employers. However, the increasing cost of medical care in the U.S. has also posed significant financial difficulties for the employers. As a result, individuals with employer-sponsored health insurance are likely to be working as full-time employees in large firms (Sullivan, Miller, Feldman, and Dowd, 1992).

One type of organization which became increasingly important as a cost cutting measure during the period of interest is the Health Maintenance Organizations (HMOs). It

is a type of managed care organization (MCO), which provides health care coverage. In this system, the care providers charge a low fee for a health care service and in return, employers who contract with HMOs ensure a steady inflow of patients. The introduction of the Health Maintenance Organization Act of 1973 forced employers with more than 25 employees to offer federally certified HMO options as well as traditional indemnity insurance plans when requested. Although HMOs had only 16% of the market in 1988, this figure increased to 31% by 1996 (Claxton, Gabel, Gil, Pickreign, Whitmore, Finder, DiJulio, and Hawkins, 2006). A large proportion of US women have attained their health insurance coverage through either their or their dependents' employers who in turn have their insurance packages to be administered by HMOs.

2.3 Infertility insurance mandates

As a way to provide coverage for the cost of infertility treatment, states individually implemented insurance mandates between 1977 and 2001 (see Table 2 in the appendix).

The extent of coverage varies across the states and these differences in the generosity of coverage stem from three main components. Firstly, there are mainly two types of mandates implemented. "Mandate to cover" regulates insurance companies to cover the infertility treatment cost regardless of the policy purchased. This is a stronger form of legislation compared to "Mandate to offer" which requires insurance companies to offer the option for consumers to purchase the coverage. Secondly, some states cover the cost of IVF while the others do not. Although IVF is not one of the most commonly used treatments, it is the most costly option (see Table 1). As a result, the differential degree of coverage for IVF by each state creates variations in the generosity of financial support given to couples. Lastly, some states implemented the mandates for HMOs whilst others excluded them from the need to cover the treatment costs. As mentioned in the previous section, HMOs play an increasingly important role in the US health insurance system. States with mandates which include HMOs, therefore, would be more likely to have a larger impact on the timing of birth than those without.

One thing to note is the lack of age limits in most states. In fact until after 2000, no states had any imposition of age restrictions. This is slightly surprising as the treatment success rate is heavily dependent on the age of the woman. Such lack of restriction may have acted as another factor to encourage women to delay giving birth.

3 Theoretical framework

Heckman and Willis (1976), illustrate a delay of birth when a couple experience a steeply rising income profile. They use a dynamic fertility model where individuals maximize discounted value of utility over her lifetime given a flow budget constraint. They assume that couples can choose, in any months of the fertile period, the level of contraception and hence the probability of conception. Under this theoretical framework, they find that couples sustain a high level of contraception until their flow of income is sufficiently high to conceive a child. Given this theory, women have incentives to delay birth in order to minimize the loss of wages, and seeing that their income profiles would improve due to hard work during their earlier years provide additional incentive for women to further delay giving birth.

When women determine when to have a child, however, they have another factor to consider, namely the biological constraint. Women could postpone giving birth if they consistently stay fertile. Women's fecundity, however, declines with age. The introduction of infertility insurance mandates reduced the price of treatment and made it possible for women to have a child for a longer period of their lives. As a result, the policy introductions effectively reduced the opportunity cost of having a child in the future. Although it is likely that not many women possess knowledge of the procedures and the costs of various infertility treatments in detail before they try to conceive through natural method, a study by Hewlett (2004) suggest that women are aware of the fact that the treatment relaxes their fertility constraint to some extent. In fact, she suggests that they may be over-estimating the effectiveness of infertility treatments as approximately

89 percent of young career driven women believe that infertility treatment enables them to have a child well into their 40s when in fact the treatment success rate drops sharply after the age of 35 (the American Society for Reproductive Medicine, 2003) . Knowing that infertility treatment could bring them a child together with the knowledge that their health insurance covers the cost of the treatment in the future, they are presented with an incentive to delay giving birth.

Although the introduction of mandates reduced the cost of future treatment, the financial burden faced by the health insurance providers was likely to have been passed on to the consumers in the form of either reduced wages if individuals obtained their insurance through their employers or higher premiums or both. There are no studies that directly investigated this effect of the infertility insurance mandates on the insurance premiums. However, several studies used other health insurance mandates to understand the impact on wages and insurance premiums. Using 1989 cross sectional data, Acs, Winterbottom, and Zedlewski (1992) note that the health insurance mandates increased premiums by 4 to 13 percent. Gruber (1994), on the other hand, studies how the state maternity mandates introduced in three states affected the wages, and concludes that the full cost of mandates was paid by women aged between 20 and 40.

Such an increase in the premiums reduces the demand for health insurance and thus the number of individuals affected by the policies are likely to have declined. A change in wages, on the other hand, generates both income and substitution effects. The reduction of wages leads women to delay birth due to an income effect. The substitution effect, however, predicts shortening of birth intervals if childbearing is complementary to leisure.

In summary, affected women are likely to face opposing incentives when determining their timing of birth and the evaluation of the policy impact requires an empirical investigation.

4 Literature

The majority of the previous literature has highlighted the impact of the mandates on older women. This is a natural choice of group as these are the women who are more likely to seek treatment. In contrast, this chapter sheds light on how the mandates changed the fertility timing preferences of younger women when they take account of the availability of cheaper infertility treatments.

The introductions of mandates are thought to have increased the use of various infertility treatments and thus are likely to have affected the birth rate. Using 1985-1999 Vital Statistics Detail Natality Data and the Census Bureau, Schmidt (2005; 2007) looks at the policy impacts on the rate of first birth, which is defined as the proportion of women with particular demographic characteristics giving first birth. Estimates from a difference-in-difference-in-differences estimator show that while the mandates did not significantly affect all US women, white women who were older than 35 experienced a significant increase in the rate of first birth (approximately 32 percent).

Bundorf, Henne and Baker(2007) study how the mandates affected the access to and the aggressiveness of ART. Due to the high cost of these treatments, women may implant multiple embryos per cycle in the hope to increase the success rate and reduce the number of cycles they need to undergo. However, such action would increase the rate of multiple births which is taxing both for maternal and fetal health. The reduction in the cost of treatment may have reduced the level of aggressiveness and multiple birth rates. Using the 1981-1999 Vital Statistics Natality Birth Data and the 1989-2000 registry data from the Society for Assisted Reproductive Technologies, they estimate the policy impact using a difference-in-differences estimator and conclude that the mandates increased the utilization of ART, however the aggressiveness of the treatment did not change even after the introduction of the mandates.

Bitler (2008), on the other hand, studies whether the mandates changed the rate of multiple births and the child health outcomes. She employs the 1981-1999 Birth Certifi-

cate Data and the 2000 Decennial Public Use Microdata Sample and 2001-2002 American Community Service Data, and finds that the probability of a twin birth increased by 10 to 23 percent. She also studies how the mandates affected various health outcomes of the newly born children. In particular, she looks at the impacts on birth weight, gestation, and 5-minute Apgar score for samples of singleton and twin births. Although no effect of mandates on these birth outcomes are found for young women aged below 30, she finds some negative impacts on the birth outcomes of the twins and singletons among older women.

Whilst these past studies have focused on how the mandates affected an older group of women, it is also possible that these state mandates influenced women who were considering a potential use of treatment in the future. In other words, the introduction of mandates may have encouraged younger women to delay giving birth. If women in the mandated states were indeed delaying their timing of birth, findings from Schmidt (2005; 2007) and Bundorf et al. (2007) not only show increase in the number of first births due to more easily accessible treatment but also reflects more women at older ages giving birth because of their planned delay of birth. This interpretation also fits with the results presented by Bitler (2008). The negative birth outcomes found among older women may be due to more women giving birth at a later age. This in turn highlights the importance of studying the timing of birth effect.

Similarly to the present paper, Buckles (2007) investigates whether women delayed births in response to these policies. Using the 1982-1999 Current Population Survey, she first looks at how the first birth rates of older women aged between 35 and 44 changed before and after the introduction of mandates using a difference-in-differences estimator and finds that women residing in mandated states increased the first birth rate by approximately 40 percent after five years of coverage. She, however, argues that estimates may simply be picking up the ability of older women to give birth due to the increasing availability of infertility treatment over time. In order to identify the cause of the delay, she then looks at how the birth rates of younger women were affected. The estimates

suggest that women aged between 22 and 25 as well as 26 and 30 both decreased the birth rates by approximately 26 percent after five years of coverage. Bundolf et al. (2007) also devotes a small section to this issue and presents similar difference-in-differences estimates which indicate that the birth rate of women aged 25-29 decreased while it increased for women aged 35-39.²

Although the evidence presented by Buckles (2007) and Bundolf et al. (2007) indicates a potential delaying effect of the mandates, they both assume that the behavior of the older cohort of women proxies for that of the younger cohort in 10 or 20 years time. Given that the lifestyles of women changed drastically over the period of observation, this may be a rather strong assumption. Moreover, the repeated cross sectional data only allows one to observe the fertility activity until the interview date and thus makes it difficult to investigate whether these young individuals are delaying births or simply not having any children at all. As a result, one may obtain an even stronger understanding of the policy impact by following how the same women responded to the mandates at different points of their lives. This paper, hence, proposes to use the framework of duration analysis using longitudinal data in order to investigate how the mandates affected the timing of birth.

5 Identification strategies

Special care is needed when studying the timing of birth using longitudinal data. Firstly, unlike other subjects of economic studies such as unemployment, a woman gives birth to the first child only once in her life time. As a result, we fail to observe the timing of first birth of the same individual with and without the policy even with the availability of longitudinal data. Secondly, women's lifestyle and fertility behavior changed drastically over the sample years.

²There is another work-in-progress research on this topic by Machado and Sanz-de-Galdeano, which was presented in the 2010 American Society for Health Economists in Cornell University. They also investigate the impact of the US infertility health insurance mandates on the timing of first birth using repeated cross sectional data.

Taking account of the first point, it is necessary to compare the influence of the mandates on the timing of birth across individuals over time. However, the second point raises a concern that one cohort of women observed in later years are not comparable to those from earlier years. Instead, I attempt to identify the policy impact by defining a comparison group which includes women are residing in the non-mandated states. These women have similar demographic characteristics to those women who were living in the mandated states but were unaffected by the policies. By comparing the timing of birth of women in mandated and non-mandated states, the policy impact is uncovered by evaluating the change in differences before and after the policy introduction dates. I, therefore, employ a difference-in-differences estimator exploiting the variation in exposure to cheaper infertility treatment across both states and time.

6 Empirical specifications

This paper carries out the analysis using the 1980-1979 PSID. The PSID records birth month and year of children born to women in the core sample. Due to the grouped nature of the data and the flexibility to incorporate a nonparametric baseline hazard, this paper employs a discrete-time proportional hazard model. This section follows materials from Jenkins (2005).

The underlying continuous-time hazard, which is the conditional hazard rate of having a first child, for the i th individual at time j is given by

$$\theta_i(j|x, \beta) = \lambda(j) \exp[x_i(j)' \beta] \quad (1)$$

$\lambda(j)$ is the baseline hazard and $x_i(j)$ denotes covariates to control for the i th individual's characteristics. In this chapter, j is measured in age years, and the discrete nature of the PSID data implies that a birth is recorded to have been given in the j th age if she gave birth on the continuous time scale of between $(j - 1)$ and j . The discrete hazard function, thus, characterizes the probability of first birth by the j th age provided that

she has not yet given birth by the $j - 1$ th age.

$$\begin{aligned}
h_i(j|x_i(j)) &= P[T = j|T > j - 1, x_i(j)] \\
&= 1 - \exp\left[-\int_{j-1}^j \theta_i(s)ds\right] \\
&= 1 - \exp[-\exp(x_i(j)'\beta + \text{Mandate}_i(j - 2)'\delta + \gamma(j))]
\end{aligned} \tag{2}$$

where

$$\gamma(j) = \ln\left[\int_{j-1}^j \lambda(s)ds\right] \tag{3}$$

In order to allow the baseline hazard to be flexible, a piece-wise constant specification is chosen. By fitting a period specific indicator variable, the baseline hazard is allowed to vary across periods. As discussed in Chapter 1, the baseline hazard captures the differential hazard rates that are caused by the lengths of childless periods prior to the observed birth. The vector of parameters $\gamma(j)$ captures the baseline hazard. The covariate vector $x_i(j)$ is assumed to be time-invariant within an interval but changes across intervals. $x_i(j)$ contains individual and state-level characteristics, a time-invariant *Policy* dummy that picks up states which introduced the infertility insurance mandates. The most important variable for our analysis is $\text{Mandate}_i(j)$. It is an indicator variable that equals one if the individual was residing in a state where the mandate had been introduced for at least two years. This definition allows individuals two years to respond to the introduction of mandates.³ To illustrate how this variable is defined, consider a woman who is living in Connecticut and is included in the sample from year 1985. Since Connecticut introduced its mandate in 1989, $\text{Mandate}_i(j)$ would be 0 for this individual for the first seven years of observation and equals 1 from 1991 onwards. The dummy equals to 1 only from 1991 in order to allow women additional two years to respond to the introduction of the

³Additional analysis allowing for three years of exposure did not change the findings discussed here.

mandates. If another woman in the same state is included in the sample from 1989, her $Mandate_i(j)$ would equal to 0 for the first two periods and 1 in the subsequent periods . In a usual framework of difference-in-differences, this dummy variable is the interaction between the *Policy* dummy and another dummy that indicates years after policies are introduced. The coefficient δ captures the policy impact which is identified by taking the differences in $\ln[-\ln(1 - h_i(j|x_i(j)))]$ between the two groups of states and evaluating the change in these differences before and after the introduction of mandates.

The discrete hazard function specified in Eq. (2) allows the set of covariates, and importantly the *Mandate* dummy, to proportionally affect the baseline hazard function. The shape of the baseline hazard, however, is common between the two groups. Since the focus of this chapter is to identify how long women delayed their first birth, an alternative hazard specification is given by

$$\begin{aligned}
h_i(j|x_i(j)) &= P[T = j | T > j - 1, x_i(j)] \\
&= 1 - \exp\left[-\int_{j-1}^j \theta_i(s) ds\right] \\
&= 1 - \exp[-\exp(x_i(j)'\beta + \gamma(j) + Mandate_i(j) \times \alpha(j))].
\end{aligned} \tag{4}$$

Eq. (4) specifies flexible duration dependence by treatment status. Given the above specification, the discrete survival function is

$$S_i(j|x_i(j)) = \prod_{k=1}^j (1 - h_i(k)) \tag{5}$$

Let c_i be an indicator variable that equals one if the spell is censored (i.e. the individual reaches the end of observation period without having any children). The contribution of the i th individual to the likelihood function is

$$\begin{aligned}
L_i &= [P(T_i = j)]^{1-c_i} [P(T_i > j)]^{c_i} \\
&= [h_i(j)S_i(j-1)]^{1-c_i} \left[\prod_{k=1}^j (1 - h_i(k)) \right]^{c_i} \\
&= \left[\frac{h_i(j)}{1 - h_i(j)} \prod_{k=1}^j (1 - h_i(k)) \right]^{1-c_i} \left[\prod_{k=1}^j (1 - h_i(k)) \right]^{c_i} \\
&= \left[\left(\frac{h_i(j)}{1 - h_i(j)} \right)^{1-c_i} \prod_{k=1}^j (1 - h_i(k)) \right]
\end{aligned} \tag{6}$$

The above analysis does not account for unobserved heterogeneity. However, Lancaster(1980) and Van den Berg (2001) point out that uncontrolled unobserved heterogeneity would cause spurious negative duration dependence as those with higher hazards tend to exit first. By taking account of the unobserved heterogeneity, the discrete hazard functions with the unobserved heterogeneity are given by

$$\begin{aligned}
h_i(j|x_i(j), \epsilon_i) &= P[T = j|T > j-1, x_i(j), \epsilon_i] \\
&= 1 - \exp\left[-\int_{j-1}^j \theta_i(s) ds\right] \\
&= 1 - \exp[-\exp(x_i(j)'\beta + \text{Mandate}_i(j)'\delta + \gamma(j) + \epsilon_i)].
\end{aligned} \tag{7}$$

and

$$\begin{aligned}
h_i(j|x_i(j), \epsilon_i) &= P[T = j|T > j-1, x_i(j), \epsilon_i] \\
&= 1 - \exp\left[-\int_{j-1}^j \theta_i(s) ds\right] \\
&= 1 - \exp[-\exp(x_i(j)'\beta + \gamma(j) + \text{Mandate}_i(j) \times \alpha(j) + \epsilon_i)].
\end{aligned} \tag{8}$$

where ϵ_i is the unobserved heterogeneity.

Assuming that the density function, $f_\epsilon(\epsilon_i)$, follows a gamma distribution, the likeli-

hood function is marginalised with respect to the unobservables. The choice of a gamma distribution as the unobserved heterogeneity distribution is rather convenient as all the relevant functions have closed form solutions. Moreover, Abbring and Van den Berg (2007) showed that when the unobserved heterogeneity is specified to proportionally affect the hazard, the unobserved heterogeneity distribution converges rapidly to a gamma distribution.⁴ The likelihood contribution for a person with a spell length j , therefore, is

$$L_i = \int \left[\left(\frac{h_i(j)}{1 - h_i(j)} \right)^{1-c_i} \prod_{k=1}^j (1 - h_i(k)) \right] f_\epsilon(\epsilon_i) d\epsilon_i \quad (9)$$

7 Data

The main data used for the analysis is the 1980-1997 Panel Study of Income Dynamics (PSID). The PSID is a nationally representative longitudinal dataset and the data is collected both at the individual and family level. The number of households surveyed initially in 1968 was 4,800 but the sample size increased to 7,000 households by 2,000 as the children of initial core sample members left households to establish their own families. From 1973, interviews were conducted over the phone and computer assisted phone surveys were introduced from 1993.

While repeated cross-sectional data have attractive features such as the large number of births and sample size, panel data presents us with several advantages. One major merit is the ability of longitudinal data to follow the same individuals. This characteristic is a crucial feature for our analysis for three reasons. Firstly, the main focus of this paper is to see how women changed their fertility behavior over time. Secondly, identifying

⁴Another possible approach is to use the nonparametric maximum likelihood estimator which fits an arbitrary distribution of unobserved frailty approximated by a set of mass points and the probability of a person at each mass point ((Heckman and Singer, 1984)). However, Baker and Melino (2000), through a Monte Carlo experiment, showed that estimating both flexible duration dependence and unobserved heterogeneity leads to a significant bias in the parameters of these components. They identify the cause of this bias to be the nonparametric maximum likelihood estimator (NPMLE) to find too many spurious mass points.

the policy impacts requires one to know whether women stayed in a particular state long enough for the mandates to have had some impacts on their timing of birth. From the cross sectional data, it is not possible to unveil this information as they only reveal in which state the individual was residing in at the time of interview. Lastly, fertility histories of women can reasonably be recovered from earlier waves. This ensures a correct selection of women, namely those without any children, into the sample.

The main period of observation is chosen to be between 1980 and 1997. As shown in Figure 2, most states introduced the mandates between mid 1980s and early 1990s. The period of observation, therefore, allows us to observe how the fertility behavior changed over years in response to the mandates. The year 1997 is chosen as the end of the observation period, since the frequency of PSID data collection changed from annually to every other year after 1997. The reduced frequency of data collection is problematic for the analysis, since information on the state of residence would be missing for the year that was not collected.⁵

Observations are organized in a person-year format. This implies that the same individual appears in the sample as many years until she either gives birth to her first child or she reaches the end of the observation period without giving birth. The width of the step, a year, is decided in order to impose less parametric structure on the baseline hazard whilst having enough birth observations. The focus of this paper is to identify the policy impacts on women’s timing of birth, and so a cohort of women who turned between 20 and 30 anytime during the observation period are included in the sample.

Some women in the PSID moved to different states during the observation period. These women have likely experienced limited influence from the mandates as their stay in a mandated state was short. It is, therefore, rather difficult to determine whether they

⁵Additional regression is estimated using data from 1980 to 2005 for robustness check. Individuals recorded to have resided in the same state before and after the missing years are assumed to have not moved across states. Although the conclusion remain the same, the magnitude of the estimates are smaller when additional years of observations are included. One possible explanation for this finding is that individuals moved across states during the years when interviews were not carried out. As a result, the mandated group may have included individuals who did not stay in the mandated state long enough to be affected, thus diluting the effect. The results are available upon request.

should be included in the treatment or the control groups as such short stays may or may not be sufficient for individuals to be affected by the mandates. As a result, only women who did not move are included in the sample.⁶ After selecting groups of individuals for the analysis, the number of individuals in the sample became 2685 contributing 10829 observations.

The dependent variable is a binary indicator that equals one if the individual gave first birth in a particular year. The demographic variables included are characteristics that are likely to affect fertility decisions such as women's educational level, ethnicity, age at the start of the observation period and its squared term. The regional and year dummies as well as state-level economic characteristics are also included in order to allow for differential characteristics across regions and years. These economic characteristics also control for the level of labour demand during the period of observation.⁷ ⁸ During the 17 year period being considered in the analysis, female labour force participation increased and women's lifestyles and preferences drastically changed. Moreover, availability of infertility treatment increased over years. As a result, individuals from later periods are more likely to give birth at an older age. In order to control for this differential fertility timing over the years, cohort dummies that take account of in which year women entered the sample are included. Marital status is not controlled in these regressions, since the status is likely to be jointly determined with fertility.

Table 3 presents the summary statistics of the PSID sample used in this chapter. The first two columns show the average characteristics of women residing in mandated and non-mandated states separately prior to the introduction of the mandates (i.e. 1970-1980). The third and fourth columns also show the statistics of the two groups from

⁶Even when these movers are included, the general conclusion remains the same but the estimated policy effect is less significant. Such reduction in the significance level may be due to the inclusion of individuals who moved for reasons other than the mandates in the affected group diluting the effect. The results are available upon request.

⁷The state-level economic indicators are calculated using the 1980-1997 March Current Population Survey (CPS). CPS is a repeated cross-sectional survey collecting information from over 50,000 households.

⁸The regressions shown in this chapter only include regional level dummies due to the limited number of observations in some states. However, the results remain unchanged even when additional regressions are estimated using state fixed effect.

1980-1997.

Comparing the statistics, these two groups of women have similar averages in most variables. However, there are minor differences in their characteristics. For example, there seems to be a higher concentration of black women in the non-mandated states throughout the period of 1970-1997. Moreover, women in mandated states are more highly educated.

The identification strategy in this chapter requires the exogenous introduction of the mandates. The employed estimator would eliminate the differences in the fertility behavior between the two groups of women as long as they experience the same trend. If, however, states introduced their policies due to the increasing demand for treatment, the demographic characteristics, and more importantly, birth trends of the two groups of women would differ. The violation of this assumption would bias the reported estimates and they would instead reflect both the policy impact as well as differential trends in fertility behavior of women.

To investigate if the disparities in the observed average characteristics translates into differential birth trends, Figure 1 display the trends of age at first birth during the pre-policy period 1970 and 1985 by mandate status and race. These trends are calculated by using the 1970-1985 NCHS's Vital Statistics Natality Birth Data which collects birth information via US birth certificates. Although there seem to be constant differences between those residing in states with and without mandates for both races, this figure indicates no disparities in trends between the two groups of women regardless of race.

Additionally, Figure 2 displays various economic characteristics such as top 10% income, median income, unemployment rates and female labour force participation during the pre-policy period for the treatment and control groups separately. Since highly educated women are the primary users of the infertility treatment, differential trends in economic characteristics would likely to unveil potential difference in the demand for treatment. These trends of economic characteristics are estimated using the 1977-1985

Current Population Survey (CPS).⁹ Again, all economic characteristics indicate common trends between the two groups for all statistics.

Although raw data suggests a common trend among women residing in mandated and non-mandated states, additional robustness checks are carried out in order to further ensure comparability of women in these two states in Section 9.

8 Results

8.1 Graphical analysis using the life table survival functions

The life table survival rates are plotted in Figure 3 for women with and without the exposure to the mandates. Points on these lines indicate the proportion of women remaining childless until a particular age. The left hand side figure shows the estimates when the entire sample is used while the figure in the middle presents estimates of women with more than 13 years of education. Moreover, since white women have more access to health insurance and thus are more likely to be affected by these mandates (Bitler and Schmidt, 2007), the right hand side figure presents the estimates for highly educated white women. All of the three figures indicate that women affected by the mandates remain childless until later stages of their lives. The observed delay seems to be more pronounced for the group of highly educated white women above the age of 30.

8.2 Regression analysis

Turning to the regression estimates, the discrete-time proportional hazard model with gamma unobserved heterogeneity discussed in Eq. (2) is estimated and the results are presented in Table 4. This specification allows for a vertical shift of the baseline hazard function proportionally to the set of demographic characteristics, but the shape of the baseline hazard function is unaltered across groups of individuals. The estimates pre-

⁹The duration of the CPS statistics is shorter as the data only reports the break down of the region of residences from 1977 onwards.

sented in Table 4, therefore, show the scaling factor of the baseline hazard function. The standard errors in the parenthesis are bootstrapped to take account of the state-level clustering.

The “Mandate” dummy selects a subgroup of women from the affected group. In particular, it picks out women from the group of affected women who were living in a state and had already been exposed to the mandates for at least two years. Its coefficient, therefore, measures the policy impact of the mandates. A negative coefficient implies a delay of birth as it indicates a smaller probability of first birth.

The first column in Table 4 shows the estimates when the treatment and control groups are defined as all women in mandated states and non-mandated states respectively.¹⁰ The estimate of the policy impact (i.e. coefficient of the “mandate” dummy) from the first column is insignificant and positive and shows no impact of the mandates.

Considering that the state-level mandates affected women with private health insurance, highly educated white women are more likely to be exposed to the policies (Bitler and Schmidt, 2007). Moreover, this group of women may face higher needs to delay birth in order to balance work and life. Reflecting this point, columns (2) to (4) in Table 4 presents the policy impact separately by various levels of educational attainment. Column (2) shows the estimated impact of women with 10 to 12 years of education. Column (3), on the other hand, presents results of women who attained 13 years or more education. Due to the limitations of the sample size, 13 years of education, which implies first year of undergraduate degree, is used as an indicator for selecting highly educated women. Results suggest differential impacts of mandates by educational attainment. As expected, significant negative policy impact are observed only for the highly educated women. Women with 10-12 years of education shortened the time until first birth instead. To see how white women are affected, column (4) in Table 4 presents estimates for highly educated white women only. Since this is a demographic group that is most likely to purchase health insurance, we expect to observe stronger impact from these women.

¹⁰The estimated coefficients of other covariates are given in Table 9.

As expected, the estimate in the last column is larger when only highly educated white women are included.¹¹

Estimates in Table 4 merely show how the baseline hazard function is shifted by a constant scale due to the introduction of the mandates. It is, however, very likely that the affected women exhibit a different baseline hazard over years in response to the mandates. As a result, Eq. (8) is estimated for highly educated women where the baseline hazard functions are separately estimated for those who were unaffected by the mandates and those who were exposed to the mandates for at least two years. Table 5 presents the estimated baseline hazard coefficients where the width of a period is a year. Since the baseline hazard can only be estimated when there are birth observations, some periods are combined assuming that the hazard rates are constant between the two periods. The first column shows $\gamma(t)$ for individuals who are unaffected by the mandates. The estimates of interests, however, are shown in the second column. These estimates present differences in the baseline hazard rates between the two groups. They suggest that when exposed to the mandates for at least two years, individuals exhibit lower probabilities of birth continuously until the 5th period.

To better illustrate how the conditional probability of giving birth to first child changed over years, Figure 4 plots the predicted hazard functions. These figures present white highly educated women's predicted conditional probability of having a first child. The left figure illustrates how affected women would exhibit differential trends if they were exposed to the mandates for two years by the time they turned 20 conditional on not having a first child until this age. Similarly, the middle and right hand side figures present the trends for women who were affected for two years by the age of 25 and 30 respectively. In all figures, the plotted predicted hazard functions clearly indicate initial

¹¹Due to the small sample size of black women, the results reported in columns (1) to (3) are estimated assuming that white and black women went through similar experiences and the that differences between the two groups stem from the constant racial factor which proportionally affects the baseline hazard. This may be a rather strong assumption, however, Figure 1 show very similar trends between the white and black women. Moreover, although not reported in this chapter, additional regressions were estimated by interacting the policy impact dummy, $Mandate_i(j-2)$, with racial characteristics. These results indicate that highly educated black women are affected in a very similar manner to white women although the sizes of the impacts are smaller.

lower conditional probabilities of first birth among women affected by the policies. The differences in the probabilities between the two groups are relatively constant until the fifth year.

The plotted predicted survival functions can be found in Figure 5. Each point on these lines indicates the probability of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. It now becomes clearer that women who were exposed to the mandates for two years by the age of 30 exhibit a greater delay of first birth compared to those who were 20 or 25. For example, when we look at the middle figure, 50% of the unaffected women were still childless at the age of 27 whilst 50% of those who were affected remain childless until the age of 28.5. On the other hand, we observe approximately a 2 year delay among women who were affected by the mandates at the age of 30 (from the age of 35 to 37). Since, less than 50% of women gave birth in the left figure, it is not possible to compare the years of delay at the median for women who were exposed to the mandates for two years by the age of 20. However, the gap is narrowing around the median, suggesting a smaller delay.

Next, Table 6 shows estimated policy impacts by the differential coverage of the mandates. As discussed in Section 2.1, each individual state adopted mandates of varying levels of generosity. If women were aware of the details of the mandates, we would expect to observe more delay among women residing in states with more generous cost coverage. The estimates presented here, however, are likely to lack precision due to the small sample size. There are only 1180 observations of highly educated women and the analysis using subsamples of these women exacerbate the small sample size problem further. This is particularly problematic when estimating the policy impact of weak mandates as states that introduced weak mandates are typically only 3-5 out the 15 mandated states. Interpretation of these results, therefore, must be done with caution.

Looking at the results in column (1) of Table 6, highly educated women seem to

respond to “Mandate to cover” more strongly by significantly delaying first birth. Since “Mandate to cover” is a more generous policy compared to “Mandate to offer”, this result matches with the prediction. Column (2), on the other hand, provides results that do not conform to the theoretical prediction. When the mandates include IVF coverage, the estimated delay is significant. However, the size of the delay seems to be larger, albeit insignificant, when IVF is not covered. In Column(3) the impacts of mandates regulating all insurance firms are compared to those that exempts some firms. Here again, women exposed to weaker mandates are responding more strongly by delaying birth.

Although the problem of the limited sample size is clearly evident in the larger standard errors among the estimated impacts for the weak mandates, further analysis is required to see if the results reflect factors other than the policy introductions.

States that are included in both the “No IVF” and “Not all insurance firms” groups are New York, Montana and West Virginia. Out of these three states, New York is the largest and is thus likely to be dominating the results. There are several potential reasons why women in New York may exhibit significant delay. One possibility is that women in New York have more access to fertility clinics compared to women in other states. However, the annual ART Success Rates Reports published by the Centers for Disease Control and Prevention reports that other states such as California and Illinois and Texas also all have equally many fertility clinics. Another potential explanation is that these women are inherently different from the others and that they would have delayed birth even in the absence of the mandates. This would be the case, for example, if these women are more career driven prior to the introduction of the mandates and thus had strong tendencies to delay birth. If this is true, the estimated policy impacts do not reflect the results of the introduction of the mandates but simply highlight the differences between women in New York and the non-mandated states.

To investigate this issue further, regressions excluding New York are run to see the size of impact New York has on the estimates. In Column (4), IVF covered states excluding New York are compared to the states which excludes IVF. The size of the estimates are

now smaller, although the “No IVF” states still seem to suggest a delaying impact. On the other hand, Column (5) reports estimates of “All insurance firms” versus “Not all insurance firms” (excluding New York). Again, New York does seem to affect the size of the estimate, but the “Not all firms” mandate coefficient is still negative and significant. From these results, New York seems to be one of the factors contributing to the large negative estimates of the weaker mandates, but is not the only cause.

The results in this section imply that women who were affected by the mandates exhibit approximately 1-2 years of delay depending on the age at which they were affected. Although plagued by small sample size problem, further analyses on the impact of mandates by differential coverage suggest potential differences other than the mandate introductions in women who were in the mandated and non-mandated states.

9 Robustness checks

9.1 Analysis on the plausibility of the results

The previous section presented evidence of delay in the timing of birth in response to the introduction of the state infertility health insurance mandates. This section describes an additional analysis that is carried out to ensure the robustness of the findings.

9.1.1 Analysis of the plausibility of the results

The estimated delay from the previous section suggests 1-2 years of delay in the timing of birth, which seems to be large. To see the plausibility of these results, Figure 6 plots trends of the average age at first births for highly educated white women in the non-mandated and mandated states between 1980-1997. These trends are calculated using the 1980-1997 NCHS’s Vital Statistics Natality Birth Data. To ensure that only states that are actually affected by the mandates contribute to the average, the mandated states include states from the year of enactment. For example, New York is included in the non-mandated group until 1989 and is defined as the mandated state only from 1990, which

was the year New York enacted its mandate.

These figures indicate that women in both groups of states experienced increases in the age at first birth during this period. However, the size of the delay is larger for women in the mandated states. In particular, whilst women in non-mandated states increased their age at first birth by approximately 1.5 years, those women residing in the mandated states went through an increase of 3.5 years. These raw statistics suggest delaying of approximately 3 more years among the women in mandated states compared to those in the non-mandated states. The national statistics, thus, support the estimated delay reported in the previous section.

9.1.2 Test for the identification assumption

Identification strategy employed in this chapter requires that the infertility insurance mandates are exogenously introduced. If, instead, these mandates were introduced in response to greater demands for infertility treatment, the employed identification strategy would not reveal the policy impact. However, there are two main reasons to believe that the introduction of the mandates do not directly reflect the demand for infertility treatment.

Firstly, insurance mandates were popular in the US between 1970s and 1990s. In fact, Jensen and Morrissey (1999) showed that the number of mandates increased by 25 folds during this period from 35 to 860.¹² Jensen and Morrissey (1999) also argued that the philosophy towards health insurance mandates differed significantly across states, and a state with a large number of mandates is more likely to pass new insurance mandates.¹³ This fact seems to suggest the state-level preference towards insurance mandates rather than the demand for infertility treatment as a driving force behind the enactments of the

¹²One of the policies that could also contribute to the delaying of birth is the mandate to cover for contraceptive methods. However, such mandates only came in effect from 1998. Maternity leave policies are also likely to be important when studying the timing of births. The first paid maternity leave was introduced in California in 2002. As our analysis only covers up until 1997, the estimated results are free of the influences from these policies.

¹³Lambert and McGuire (1990) also show that the states with many mandates were more likely to introduce a new mandate for mental health.

mandates.

Secondly, the lobbying activity for the infertility insurance mandates is mainly carried out by a non-profit organization, RESOLVE (Fulwider, 2009). RESOLVE actively seeks coverage for infertility treatment on local, state and national levels. It is founded in 1974 and run by a group of volunteers broadly consisting of both health care professionals and individuals who have had personal experiences with infertility and/or adoption. Although there is a concern that RESOLVE's choice of states is driven by the underlying demand for the infertility treatment within a state, there are several other states, where the lobbying activities took place but were not fruitful. Examples of these states include Virginia , which went through 6 attempts to enact the infertility mandate since 1990 (Audit and of the Virginia General Assembly, 2008), as well as Florida that holds the second largest number of infertility clinics in the US (the Centers for Disease Control and Prevention, 2000). Other such states also include Nebraska, Michigan, Maine, Pennsylvania, Arizona, Maryland, Missouri, Kansas, Michigan and Oklahoma.

The existence of these states with unsuccessful attempts highlights the potential importance of several factors other than the demand for infertility treatment, namely the opposition forces from the health insurance providers as well as the concerns among the policymakers regarding the moral ethics involved with the infertility treatment. Indeed, the two case studies in the state of Illinois and Nevada carried out by Fulwider (2009) reveal that the main debates among the policymakers regarding the passing of the infertility insurance bill involved the potential cost of the mandate towards the health insurance providers and employers. In addition, some senators raised the issues of moral and ethical dilemma associated with infertility treatment. These policymakers argued that ART procedure resembles that of selective abortion, since it involves selections of eggs for the purpose of implantation and abortions in the case of multiple pregnancies.

The background information strongly suggests that the infertility mandates were exogenously introduced. Nonetheless, Table 7 presents results from a placebo test, where Eq. (7) is estimated using the pre-policy period 1970-1985 PSID data. Looking at a pre-

introduction period assures similarities in fertility behavior between the affected and not affected groups and hence ensures the robustness of the identification strategy. Since all except for West Virginia introduced their mandates after 1985, the period of observation presents women’s fertility behavior in the absence of the policy interventions. Moreover, by the year 1985, various infertility treatments were already available. The selected period, therefore, allows us to see if highly educated women had differential preferences towards their birth timings when various treatments could be purchased without the health insurance coverage. Since West Virginia had already introduced the mandate in 1977, it is excluded from the estimation. The result from column (1) are reassuring with regard to the exogeneity of the policy introductions as the coefficient on the *Mandate* dummy is small and statistically insignificant. In addition, Figure 7 plots the predicted hazard functions by treatment status and clearly indicates that these two groups exhibited a very similar trend in the absence of mandates.

Although the robustness checks so far seem to indicate no differences in the timing of birth between mandated and non-mandated states, estimated results in Section 8 raised a concern that there may be underlying differences between women residing in states with weak mandates and the others. If these women were indeed inherently different from the others, it is likely to observe the differential birth trends even before the introduction of the mandates. In order to test this, policy impacts are separately estimated for differential coverage. However, even when the policy variables are estimated separately by those living in “Mandate to cover” and “Mandate to offer”, no evidence of differences prior to the introductions of mandates are found (column (2)). Moreover, estimated results are generally small, positive and insignificant and thus does not indicate any differences between the states with and without the IVF coverage(column (3)). Additionally, column (4) presents results for states that regulated all firms to follow the mandated states vs those that excluded some insurance firms. Again, there are no differential timing of birth prior to the introduction of these mandates.

9.1.3 Test for the assumptions in the empirical specification

Individuals who are found to be in the initial period at the age of 20 were likely to have faced different hazard rates compared to those who were aged 25 at the start of the observation period. In this chapter, it is assumed that such differences are controlled for by the inclusion of the initial age variable. In other words, the differences in the initial condition are assumed to be reflected in the proportional alteration of the baseline hazard. At the same time, this implies an additional assumption that the differences in the initial condition can be controlled for solely by observed characteristics.

In order to test for these assumptions, only individuals who enter the sample at the age of 20 are included. This makes sure that every woman is found to be in the initial period under the same condition. However, this reduces the sample size. As a result, observations with one or two missing years during the sample period are still included, filling these missing observations as long as their region of residence before and after the missing years are the same. This is likely to reduce the size of the estimates if women were moving during these unobserved years for reasons other than the mandates. On the contrary, this may amplify the size of the estimates if people moved to take advantage of the mandates.

Although the lack of observations restricts our analysis only to those who were affected from the age of 20, the results presented in Table 8 confirm the conclusion drawn in Section 8. Just as the results in Table 5, the coefficients in the second column, which show the differences in the hazard rate in each period between those who are affected and unaffected, indicate a delay of birth until the 5th period. Moreover, just as before, the differences are statistically significant in periods 2 and 4. Additionally, we now observe a significant reduction in the hazard also in the 5th period. Figure 8 presents the survival functions which are plotted using the estimates from Table 8. This figure suggests approximately a year delay at the median. The size of the delay is similar to the result in the main analysis for those who were affected from the age of 20 (see the left side figure in Figure 5).

10 Conclusions

This paper investigates the impact of the US infertility state mandates on the timing of first birth. A discrete-time proportional hazard model is estimated allowing for a flexible nonparametric baseline hazard as well as gamma unobserved heterogeneity.

In contrast to the past literature, which has focused on how these mandates affected older women, the present paper looks at policy impacts on younger women. In other words, while women who undergo infertility treatment are generally older, it proposes the existence of a potential effect on younger cohorts of women who were likely to have been planning to have a child in the future. Facing the difficulties in balancing work and life, these women may have incorporated the availability of cheap and thus more accessible infertility treatment into their life cycle plan. If this is the case, we should observe a delay in the time to first birth among the affected women.

The results from the discrete-time proportional hazard model indicate an insignificant effect of the mandates when the entire sample is included and the effect is assumed to be the same across educational group. However, a significant negative effect of these mandates on the timing of first birth is observed among white women with more than 13 years of education. Moreover, when separate baseline hazard functions are estimated, evidence suggests that individuals affected by the mandates for at least two years were delaying birth. Moreover, the size of the delay depended on the age at which these women became exposed to the mandates. For example, at the median of the survival function, affected white women are estimated to have delayed their first birth for 1 year if they were exposed to the legislation for two years by the the age of 20 or 25. The size of delay becomes even larger when they were affected at the age of 30. In particular, these women are observed to have delayed their first birth for 2 years. The estimated policy impact translates to approximately 14 percent increase in the number of women who face infertility. This implies an increase of approximately 0.37 million infertile women.

There are two potential explanations for why we observe stronger impacts among the

women exposed to the mandates at older ages. Firstly, the older childless women had already delayed birth possibly for career or educational reasons and thus are likely to be the sample of women who had a stronger incentives to delay birth in order to balance work and life. Secondly, the notion of pregnancy and timing of birth is likely to be more of a serious issue for women who were at the age of 28 than those who were younger.

Results broken down by the level of coverage indicated that women in weaker mandated states seem to be responding more strongly by delaying birth. This raises a concern as it may indicate an underlying cause of the delay observed other than the state-level infertility health insurance mandates. However, the small sample size, reflected in the large standard errors, raises a concern over the precision of the estimates.

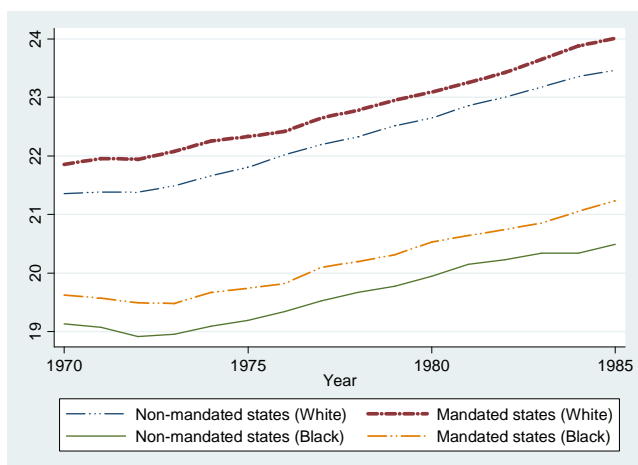
In order to confirm no differential trends between the affected and unaffected groups, robustness checks were carried out using the pre-policy period (1970-1985) data. If affected women were different from the other women, such differences are likely to be observed prior to the introduction of the mandates. However, no matter how we divide the sample, we observe no differences between the two groups of women and thus indicating the robustness of the delaying effect found in this paper.

Two further assumptions regarding the initial conditions of individuals in the sample are tested by using only those women who turned 20 at the beginning of the observation period. Although the smaller sample size only allows us to study the effect among individuals who were affected for two years by the age of 20, the results from this sample draws the same conclusion as those in the main analysis.

This paper demonstrate that the introduction of infertility insurance state mandates not only affected those who are directly targeted, but had a wider policy impact on the timing of birth. Further research is also needed in order to uncover how the timing of second birth was affected by these mandates. Due to the delay of first birth, women may have had their second child significantly after the age of 35 further increasing the health risks for both mothers and children. Moreover, such an analysis would inform us whether the infertility health insurance mandates affected total fertility rate.

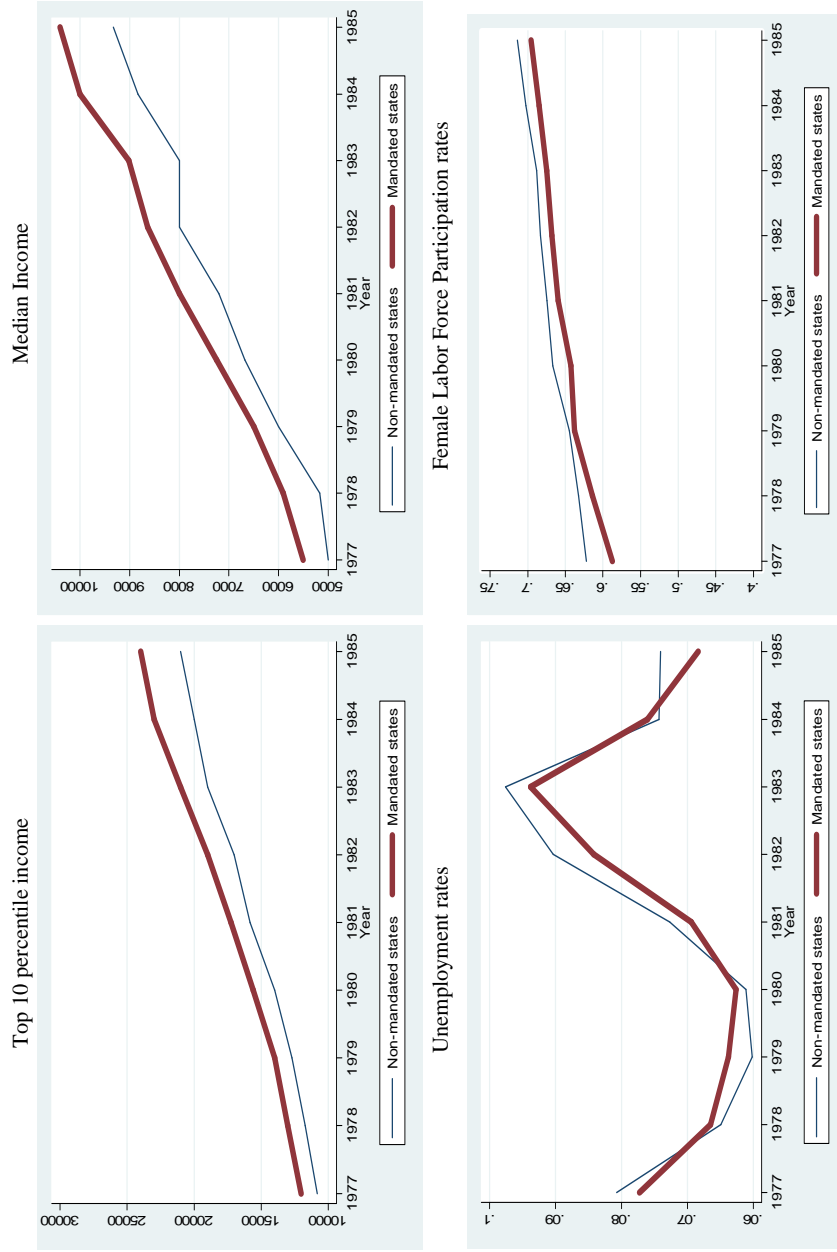
10.1 Chapter Two: Figures and Tables

Figure 1: Trends of mean age at first birth by mandate status and race



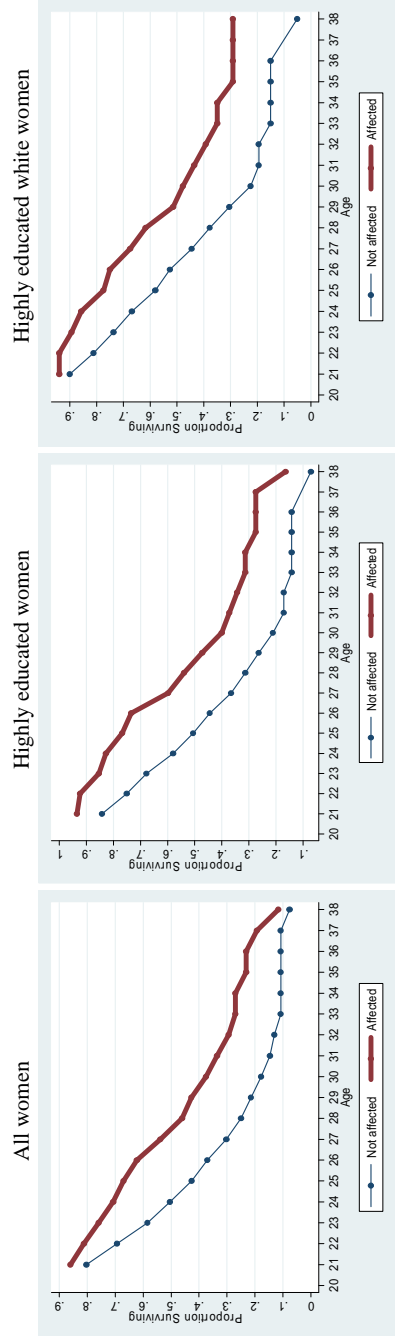
Notes: This figure presents the trends of age at first birth by race and mandates status. Statistics are calculated for the period prior to the introduction of mandates (1970-1985). The ages at first birth are computed using the NCHS's Vital Statistics Natality Birth Data.

Figure 2: Trends of economic characteristics by mandate status



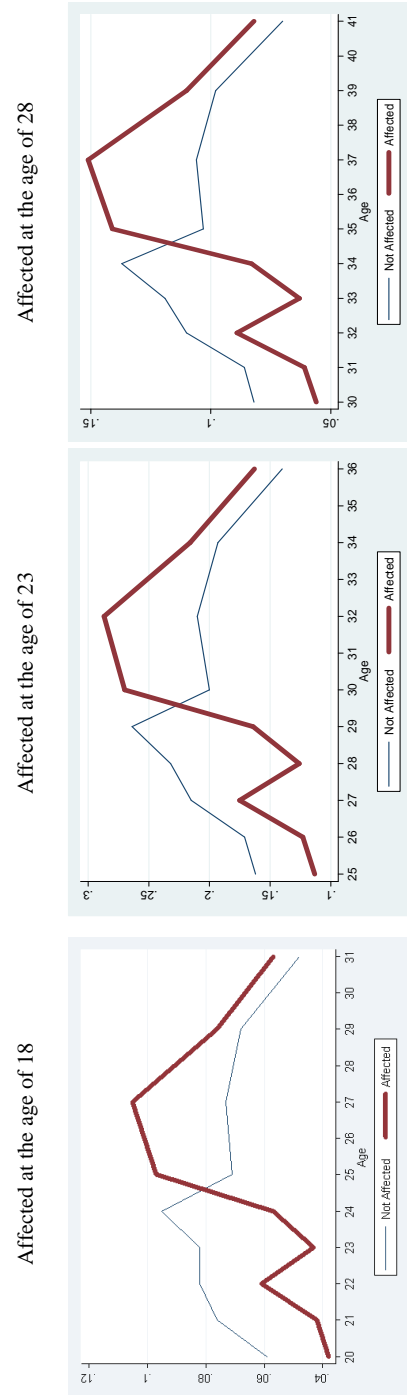
Notes: These four figures present pre-policy period economic characteristic trends by mandate status. Data is taken from the 1977-1985 Current Population Survey. In each figure, the thin and thick lines show the trends experienced by the non-mandated and mandated states respectively.

Figure 3: Life table survival functions



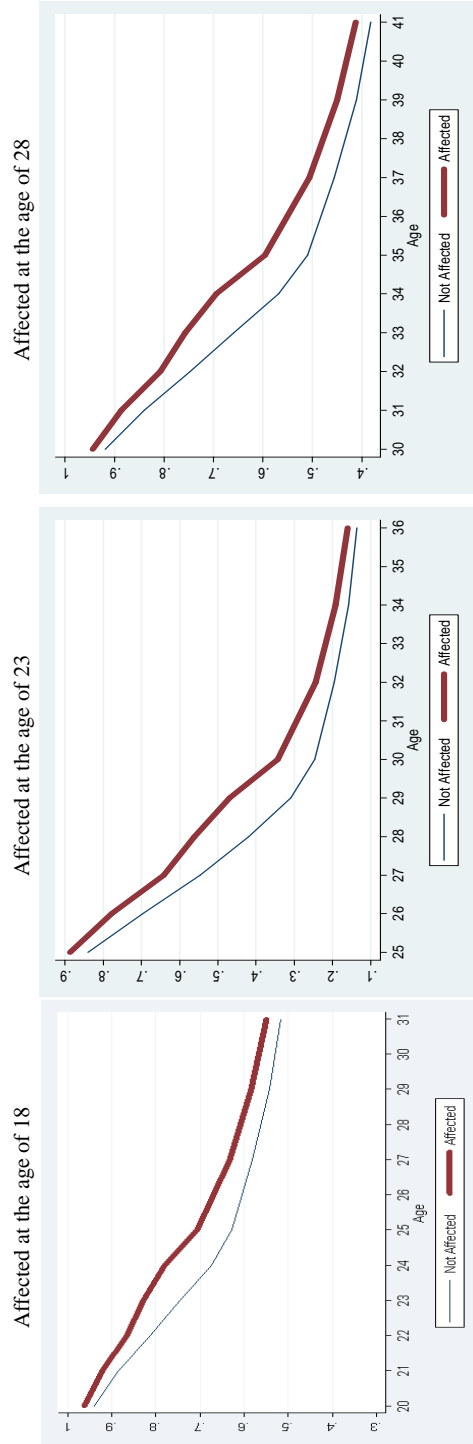
Note: In each of these figures, the life table estimates of the survival functions are plotted for two groups (i.e. affected and not affected groups) separately. The left figure shows the survival functions when all women are included. On the other hand, the middle and left figures each presents the graph for highly educated women and highly educated white women respectively. Data employed is the 1980-1997 Panel Study of Income Dynamics. Every point on these lines displays the proportion of women remaining childless until a particular age. The thin lines show survival rates for women who are unaffected by the mandates whilst the thick lines present that for women who were exposed to the mandates for at least two years by the age of 20.

Figure 4: Predicted hazard functions(White and highly educated women)



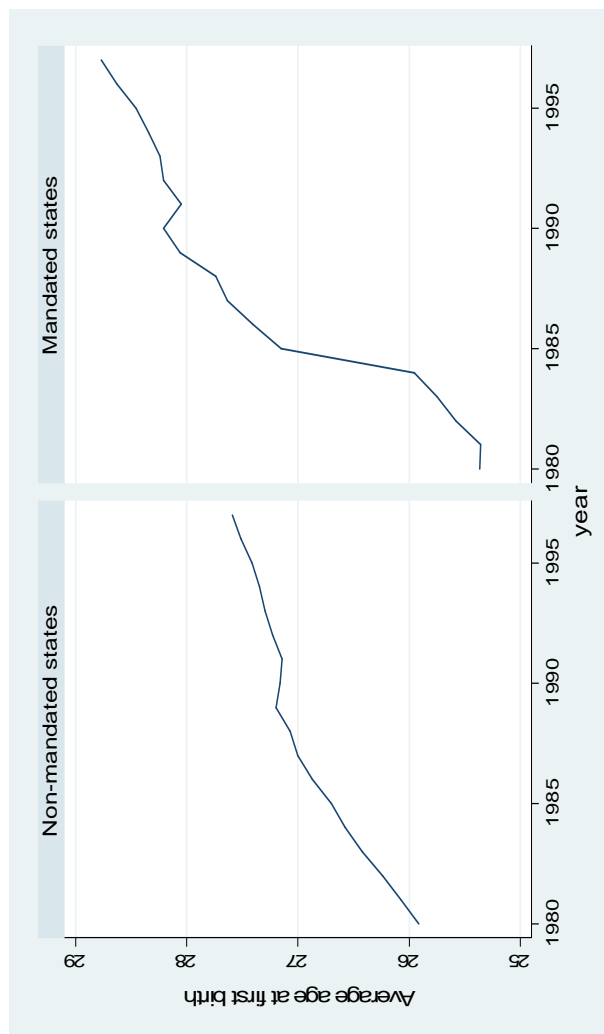
Notes: The predicted hazard functions of white highly educated women are plotted for two groups separately. Each figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of 20, 25 and 30 respectively. Every point on these lines displays the conditional probability of having a first child at a particular age. The thin lines show the predicted hazard for women who are unaffected by the mandates whilst the thick lines present the predicted hazard for women who were exposed to the mandates for at least two years. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 5 and Table 9.

Figure 5: Survival functions (White and highly educated women)



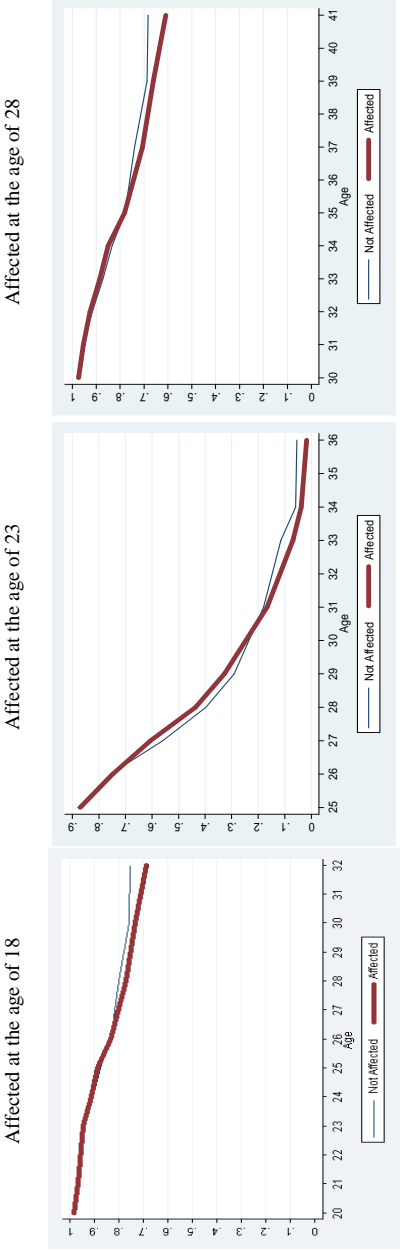
Notes: Above figures present the survival functions of white highly educated women. Each figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of 20, 25 and 30 respectively. Points on these lines indicate the probabilities of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The estimates for the baseline hazard and covariates are included in Table 5 and Table 9 in the appendix.

Figure 6: Trends of mean age at first birth by mandate status (White and highly educated women)



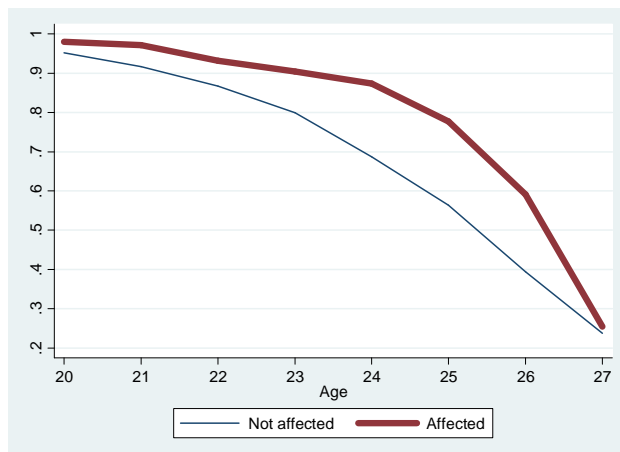
Note: These figures plot trends of age at first birth for the highly educated white women during the period of 1980-1997. The left figure plots the trends of the non-mandated states. The right figure, on the other hand, shows the trend of the mandated states. The states are included in right figure from the year in which they enact the mandates. For example, New York is defined to be a non-mandated state until 1989 and is included in the mandated state group only from 1990. Statistics are calculated using the NCHS's Vital statistics natality data.

Figure 7: Survival functions (Pre-policy period analysis)



Note: These figures display the survival functions calculated using the pre-policy period Panel Study of Income Dynamics data (1970-1985). For robustness check, an arbitrary year was chosen to assume the introduction of the mandates. Each figure compares the survival rates of unaffected and affected women at the initial age of 18, 23 and 28 respectively. Every point on each line indicates the probability of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those for women who were assumed to have been exposed to the pseudo policies. These probabilities are estimated using the discrete-time proportional hazard estimates with piecewise constant baseline hazard and gamma unobserved heterogeneity.

Figure 8: Robustness check: survival functions (White and highly educated women)



Notes: Above figure presents the survival functions of white highly educated women when only those who turned 20 are included in the sample. The figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of 18. Points on these lines indicate the probabilities of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The estimates for the baseline hazard and covariates are included in Table 8 and Table 9.

Table 1: Treatment options, success rates and costs

Treatment	Description	Success rates	Cost	Multiple births
Fertility Drugs	Regulate reproductive hormones and trigger the release of more eggs per cycle.	20-60 percent (often with IUI)	Clomiphene: Minimum \$50 per cycle Gonadotropins:\$2000-\$5000 including tests, drugs and medical check ups.	Yes (8-10 percent twin rate for Clomiphene, 15 percent for Gonadotrophin)
Surgery	Unblocking the fallopian tubes or removing endometrial scarring, fibroids, or ovarian cysts.	40-60 percent (if treated for endometriosis and scar tissues) 10-90 percent (if treated for blocked fallopian tubes)	\$3,000- \$10,000	
Intrauterine insemination (IUI)	A concentrated dose of sperm is injected into the uterus or fallopian tubes with a catheter.	5 to 20 percent	\$300-\$700 (\$1,500-\$4,000 including medication and ultrasound monitoring).	Yes if fertility drug is also used in conjunction to this method.
In vitro fertilisation (IVF)	Eggs removed from the ovaries are fertilised with sperm in a laboratory, and the resulting embryos are transplanted back to the uterus.	28 and 35 percent	\$8,000-\$15,000 per cycle \$50000 until success or \$44,000 and \$211,940 (Neumann, Gharib, and Weinstein (1994))	Yes(20-25% chance)
Gamete intrafallopian transfer (GIFT)	Eggs and sperm are harvested and mixed together in a lab. The mixture is surgically injected into the fallopian tubes so fertilisation can happen naturally inside the body.	25 to 30 percent	\$8 000 - \$15 000	Yes
Intracytoplasmic sperm injection (ICSI)	A single sperm is injected into a single egg and the resulting embryo is transplanted into the uterus.	35 percent	\$10,000 - \$17,000 per cycle	Yes
Donor sperm	Donated sperm is used during an IUI treatment. IVF techniques can also be carried out using donor sperm.	20 to 26 percent (when used with IVF)	\$200-\$3000 per unit of semen	(Yes, if other treatment is used together)
Egg (or embryo) donation	An egg (or embryo) donated by another woman is mixed with sperm and implanted in the recipient's uterus.	43 percent (when used with IVF)	\$4,000 -\$5,000	(Yes. 20-25% chance)
Surrogacy	Another woman carries a couple's embryo, or a donor embryo, to term.	Not Available	\$15,000- \$50,000	
Zygote Intrafallopian Transfer (ZIFT)	Similar to GIFT but the doctors make sure the egg is fertilized before implanting it into the womb.	25 to 30 percent	\$8 000 -\$15 000	Yes

Source:Getting Pregnant (2009) *Sperm Donation*, last revised 2009, Retrieved August 20, 2009 from http://www.wdxcyber.com/sperm_donation.html

BabyCenter (2009) *Fertility treatment: Your options at a glance*, last revised 2009, Retrieved August 20, 2009 from http://www.babycenter.com/0_fertility-treatment-your-options-at-a-glance_1228997.bc?page=1

Table 2: States with mandate coverage

State	Year law passed	Mandate to		IVF is		Law applies to			Upper age limit
		cover	offer	included	excluded	All firms	Non-HMOs	Only HMOs	
Arkansas	1987	Y	N	Y	N	N	Y	N	
California	1989	N	Y	N	Y	Y	N	N	
Connecticut	1989	2005 onwards	Before 2005	Y	N	N	Y	N	Below 40 (2005~)
Hawaii	1987	Y	N	Y	N	Y	N	N	
Illinois	1991	Y	N	Y	N	Y	N	N	
Louisiana	2001	Y	N	N	Y	Y	N	N	
Maryland	1985	Y	N	Y	N	Y	N	N	
Massachusetts	1987	Y	N	Y	N	Y	N	N	
Montana	1987	Y	N	N	Y	N	N	Y	
New York	1990	Y	N	N	Y	N	Y	N	21-44 (2002~)
New Jersey	2001	Y	N	Y	N	Y	N	N	Below 46
Ohio	1991	Y	N	Before 1997	1997 onwards	N	N	Y	
Rhode Island	1989	Y	N	Y	N	Y	N	N	25-40 (2006~)
Texas	1987	N	Y	Y	N	Y	N	N	
West Virginia	1977	Y	N	N	Y	N	N	Y	

Sources: Bitler (2008), Resolve (2008), and The New York Times (2002)

Notes: This table presents the states that had implemented the state-level mandates and summarizes the extent of their coverage. Mandate “to cover” is a type of mandate that requires insurance companies to cover the infertility treatment cost regardless of the insurance policies purchased. On the other hand, mandate “to offer” simply regulates insurance providers to offer infertility insurance policies to customers.

Table 3: Summary statistics

	1970-1980				1980-1997			
	Treatment		Control		Treatment		Control	
	Mean	S.D	Mean	S.D	Mean	S.D	Mean	S.D
<i>Age in the first period</i>	21.65	2.90	21.09	3.05	22.47	3.16	21.90	2.71
<i>Birth (1 if birth observed)</i>	0.13		0.13		0.10		0.18	
<i>State-level economic indicators</i>								
Median annual income	7033.63	1593.85	6229.25	1646.40	14668.93	3806.48	13440.55	3859.00
Top 10 percentile annual income	17621.79	4195.27	16032.85	4106.10	38772.77	9735.18	35580.74	9585.53
Female labor force participation rate	0.46	0.04	0.46	0.05	0.54	0.04	0.54	0.05
Female unemployment rate	0.07	0.02	0.07	0.02	0.07	0.02	0.07	0.03
<i>Ethnicity dummies</i>								
White	0.92		0.87		0.90		0.88	
Black	0.07		0.13		0.09		0.11	
<i>Education dummies</i>								
Highest grade attended 1-5	0.001		0.004		0.01		0.01	
Highest grade attended 6-8	0.001		0.01		0.00		0.01	
Highest grade attended 9-12	0.43		0.47		0.30		0.39	
Highest grade attended 13 or more	0.56		0.52		0.70		0.59	
<i>Region of Residence dummies</i>								
New England	0.19		0.00		0.17		0.02	
Mid-atlantic	0.20		0.20		0.20		0.23	
Mid-west	0.27		0.37		0.21		0.26	
South Atlantic	0.03		0.18		0.04		0.22	
East South	0.00		0.09		0.00		0.09	
West South	0.11		0.03		0.14		0.03	
Mountain	0.00		0.07		0.00		0.08	
Pacific	0.20		0.05		0.24		0.07	
<i>Starting year dummies</i>								
1 if the observation enters in the sample in 1970/1980	0.38		0.43		0.40		0.34	
1 if the observation enters in the sample in 1971/1981	0.06		0.05		0.05		0.04	
1 if the observation enters in the sample in 1972/1982	0.11		0.08		0.06		0.05	
1 if the observation enters in the sample in 1973/1983	0.08		0.08		0.05		0.06	
1 if the observation enters in the sample in 1974/1984	0.07		0.08		0.07		0.05	
1 if the observation enters in the sample in 1975/1985	0.07		0.08		0.06		0.06	
1 if the observation enters in the sample in 1976/1986	0.09		0.07		0.04		0.06	
1 if the observation enters in the sample in 1977/1987	0.06		0.03		0.05		0.04	
1 if the observation enters in the sample in 1978/1988	0.05		0.03		0.02		0.04	
1 if the observation enters in the sample in 1979/1989	0.03		0.05		0.05		0.05	
1 if the observation enters in the sample in 1980/1990	0.01		0.01		0.06		0.05	
1 if the observation enters in the sample in 1991					0.01		0.04	
1 if the observation enters in the sample in 1992					0.02		0.03	
1 if the observation enters in the sample in 1993					0.02		0.03	
1 if the observation enters in the sample in 1994					0.02		0.02	
1 if the observation enters in the sample in 1995					0.01		0.02	
1 if the observation enters in the sample in 1996					0.01		0.01	
1 if the observation enters in the sample in 1997					0.01		0.01	
<i>Year dummies</i>								
1 if observed in 1970/1980	0.07		0.07		0.06		0.05	
1 if observed in 1971/1981	0.07		0.07		0.06		0.05	
1 if observed in 1972/1982	0.07		0.07		0.00		0.00	
1 if observed in 1973/1983	0.06		0.07		0.06		0.05	
1 if observed in 1974/1984	0.08		0.09		0.06		0.05	
1 if observed in 1975/1985	0.09		0.10		0.05		0.06	
1 if observed in 1976/1986	0.10		0.10		0.06		0.06	
1 if observed in 1977/1987	0.11		0.10		0.06		0.06	
1 if observed in 1978/1988	0.11		0.10		0.05		0.05	
1 if observed in 1979/1989	0.13		0.12		0.06		0.06	
1 if observed in 1980/1990	0.12		0.12		0.07		0.06	
1 if observed in 1991					0.06		0.06	
1 if observed in 1992					0.06		0.06	
1 if observed in 1993					0.07		0.07	
1 if observed in 1994					0.07		0.07	
1 if observed in 1995					0.05		0.06	
1 if observed in 1996					0.06		0.07	
1 if observed in 1997					0.04		0.05	
Number of observations	2103		3552		3997		6832	
Number of individuals	586		1015		1001		1684	

Note: This table reports the averages and standard deviations of variables taking account of the survey data structure of PSID. Treatment group includes women who were residing in states that introduced mandates sometime during the observation period. The first two columns report the summary statistics of variables from the pre-policy period data (i.e. 1970-1980) while the third and fourth columns show that of the post-policy period data (i.e. 1980-1997).

Table 4: Estimates of mandates effect

	(1)	(2)	(3)	(4)
	All Women	By education		
		10≤Education≤12	13≤Education	
	All race	All race	All race	White
Mandate (Policy×After)	0.03 (0.12)	0.30* (0.17)	-0.38** (0.17)	-0.54** (0.22)
Policy	0.14 (0.42)	0.12 (0.33)	0.16 (0.46)	0.08 (0.61)
LR test of gamma variance	12.40***	2.28*	1.60	15.30***
Number of women observed	2685	1339	1180	839
Observations	10829	4794	4925	3662

Notes: This table displays key policy impact variables from discrete-time proportional hazard estimates with heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed in a piece-wise constant baseline hazard and gamma unobserved particular year and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 9 in the appendix. Covariates included are: age of individuals in the first year of observation and its squared term, race, education and region of residence dummies, state-level characteristics, year fixed effects and start year dummies. The flexible baseline hazard is assumed to be common between the treatment and control groups. Column (1) shows regression results when all women in the sample are included. Column (2) shows results estimated using a sample of women with 10 to 12 years of education. Column (3) shows results for women with more than 13 years of education. Column (4) shows results for white women with more than 13 years of education. Standard errors are bootstrapped to take account of state-level clustering and are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Estimated baseline hazard

Periods (t)	Coefficients	Periods(t)	Coefficients
		Mandate_period1	-0.58 (0.46)
period2	0.06 (0.13)	Mandate_period2	-0.55*** (0.20)
period3	0.32* (0.18)	Mandate_period3	-0.42 (0.36)
period4	0.40* (0.23)	Mandate_period4	-0.87*** (0.31)
period5	0.55* (0.30)	Mandate_period5	-0.73 (0.48)
period6/7	0.25 (0.33)	Mandate_period6/7	0.14 (0.27)
period8/9	0.27 (0.44)	Mandate_period8/9	0.19 (0.39)
period10/11	0.20 (0.57)	Mandate_period10/11	-0.07 (0.23)
period12/15	-0.16 (0.63)	Mandate_period12/15	-0.02 (0.37)

Notes: This table displays the baseline hazard estimates from discrete-time proportional hazard model with piece-wise constant baseline hazard and gamma unobserved heterogeneity. The first column shows the piece-wise constant baseline hazard for the unaffected individuals whereas the second column includes the difference in hazard between the affected and unaffected individuals. Number of individuals in the sample is 1180 contributing binary responses of 4925. LR test of gamma variance reports a chi squared statistics of 1.713*. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the covariates are included in Table 9 in the appendix. Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. p<0.01, ** p<0.05, * p<0.1 ***

Table 6: Policy impacts by differential coverage

	Highly educated women only				
	(1)	(2)	(3)	(4)	(5)
	Cover vs Offer	IVF vs no IVF	All firms vs Not all firms	IVF vs no IVF (excluding New York)	all firms (excluding New York)
Mandate_Cover	-0.43*				
	(0.26)				
Mandate_Offer	-0.33				
	(0.21)				
Policy_Cover	0.1				
	(0.47)				
Policy_Offer	0.41				
	(0.73)				
Mandate_IVF covered		-0.18*		-0.18*	
		(0.11)		(0.11)	
Mandate_IVF not covered		-0.71		-0.35	
		(0.44)		(0.29)	
Policy_IVF covered		-0.01		-0.02	
		(0.50)		(0.17)	
Policy_IVF not covered		0.47		0.22	
		(0.68)		(0.28)	
Mandate_All insurance firms			-0.13		-0.12
			(0.15)		(0.12)
Mandate_Not all insurance firms			-1.01**		-0.67**
			(0.40)		(0.31)
Policy_All insurance firms			0.09		0.04
			(0.44)		(0.20)
Policy_Not all insurance firms			0.31		0.01
			(0.62)		(0.32)
LR test of gamma variance	3.60**	1.53	3.55**	1.34	2.51*
Number of women observed	1180	1180	1180	1110	1110
Observations	4925	4925	4925	4635	4635

Notes: This table displays key policy impact variables estimated separately by the characteristics of the mandate.

These results were estimated using the discrete-time proportional hazard model with piece-wise constant baseline hazard and gamma unobserved heterogeneity. The dependent variable is a dummy which equals to one if birth observed 0 otherwise.

The estimates for the baseline hazard and covariates are included in the appendix (Table 9)

Table 7: Test for differential trends between the mandated and non-mandated states

	Highly educated women only			
	(1)	(2)	(3)	(4)
	13≤Education	Cover vs Offer	IVF vs no IVF	All firms vs Not all firms
Mandate	-0.01 (0.22)			
Policy	0.02 (0.19)			
Mandate_Cover		-0.14 (0.28)		
Mandate_Offer		0.07 (0.26)		
Policy_Cover		0.03 (0.21)		
Policy_Offer		-0.14 (0.28)		
Mandate_IVF covered			-0.14 (0.26)	
Mandate_IVF not covered			0.12 (0.29)	
Policy_IVF			-0.00 (0.23)	
Policy_IVF not covered			0.06 (0.30)	
Mandate_All insurance firms				0.14 (0.27)
Mandate_Not all insurance firms				-0.12 (0.33)
Policy_All insurance firms				-0.10 (0.26)
Policy_Not all insurance firms				0.22 (0.29)
LR test of gamma variance	47.01 ***	46.18 ***	46.51 ***	55.68 ***
Number of women observed	939	939	939	939
Observations	4257	4257	4257	4257

Note: This table displays regression results from the robustness analyses, which are estimated using the pre-policy 1970-1985 PSID. The estimates shown are the key variables from the discrete-time proportional hazard model with piece-wise constant baseline hazard and gamma unobserved heterogeneity. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. Covariates included are: age of individuals in the first year of observation and its squared and cubed terms, race, education and region of residence dummies, state-level economic indicators, state fixed effect and start year dummies. In column (1), policy impacts are estimated for all highly educated women. Column (2) presents results of differential policy impacts among highly educated women affected by "mandate to cover" and "mandate to offer". Column (3) show results for states with and without the IVF coverage and Column (4) includes that for women residing in mandates which regulated all health insurance firms and those that excluded some firms. Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. *** p<0.01, **p<0.05, *p<0.1

Table 8: Test for the assumptions regarding the sampling scheme

Periods (t)	Coefficients	Periods(t)	Coefficients
		Mandate_period1	-0.51 (0.507)
period2	-0.27 (0.180)	Mandate_period2	-0.99*** (0.343)
period3	0.13 (0.181)	Mandate_period3	0.15 (0.254)
period4	0.53** (0.245)	Mandate_period4	-0.66* (0.399)
period5	1.15*** (0.245)	Mandate_period5	-1.15*** (0.402)
period6/7	1.42*** (0.314)	Mandate_period6/7	-0.17 (0.268)
period8/9	1.98*** (0.449)	Mandate_period8/9	0.11 (0.340)
period10/11	2.48*** (0.644)	Mandate_period10/11	0.73** (0.306)
period12/15	2.97*** (0.794)	Mandate_period12/15	-0.14 (0.413)

Notes: This table shows results estimated by using a sample of women who were 20 in the initial period. The first column shows the piece-wise constant baseline hazard for the unaffected individuals whereas the second column includes the difference in hazard between the affected and unaffected individuals. LR test of gamma variance reports a chi squared statistics of 10.82***. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 9 in the appendix. Number of individuals in the sample is 1101 contributing binary responses of 4125. Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. p<0.01, ** p<0.05, * p<0.1 ***

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11 Appendix

Table 9: All estimates (Tables 4-6)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
<i>Period</i>										
Period 2	0.03 (0.10)	-0.01 (0.10)	0.06 (0.14)	0.28** (0.13)		0.11 (0.14)	0.06 (0.14)	0.10 (0.14)	0.08 (0.16)	0.12 (0.10)
Period 3	0.23 (0.17)	-0.01 (0.19)	0.36* (0.21)	0.94*** (0.22)		0.45** (0.19)	0.33* (0.18)	0.43** (0.19)	0.32 (0.22)	0.39** (0.18)
Period 4	0.38 (0.24)	0.19 (0.24)	0.38 (0.31)	1.08*** (0.32)		0.51* (0.28)	0.35 (0.27)	0.49* (0.28)	0.31 (0.29)	0.42 (0.31)
Period 5	0.43 (0.30)	0.01 (0.29)	0.55 (0.39)	1.71*** (0.43)		0.72** (0.33)	0.50 (0.31)	0.69** (0.33)	0.50 (0.35)	0.64** (0.30)
Period 6	0.44 (0.35)	0.22 (0.38)	0.18 (0.44)	1.37*** (0.50)		0.39 (0.38)	0.13 (0.36)	0.36 (0.37)	0.12 (0.43)	0.30 (0.45)
Period 7	0.71* (0.40)	0.31 (0.48)	0.70 (0.50)	1.52*** (0.57)		0.94** (0.43)	0.65 (0.41)	0.90** (0.41)	0.65 (0.46)	0.84 (0.54)
Period 8	0.75 (0.48)	0.49 (0.49)	0.42 (0.61)	1.64** (0.66)		0.69 (0.54)	0.37 (0.53)	0.67 (0.50)	0.39 (0.52)	0.62 (0.60)
Period 9	0.76 (0.55)	0.27 (0.63)	0.57 (0.65)	2.15*** (0.71)		0.87 (0.57)	0.51 (0.54)	0.85 (0.54)	0.54 (0.57)	0.80 (0.49)
Period 10	0.90 (0.58)	0.47 (0.59)	0.63 (0.80)	2.02** (0.85)		0.94 (0.71)	0.56 (0.66)	0.92 (0.68)	0.47 (0.61)	0.74 (0.63)
Period 11	0.84 (0.68)	0.62 (0.69)	-0.02 (0.86)	1.47* (0.83)		0.33 (0.79)	-0.09 (0.74)	0.32 (0.75)	-0.04 (0.71)	0.27 (0.70)
Period 12	0.91 (0.74)	0.71 (0.86)	0.09 (0.96)	1.79** (0.85)		0.45 (0.85)	0.02 (0.75)	0.42 (0.79)	0.07 (0.74)	0.38 (0.70)
Period 13	1.21 (0.73)	0.30 (0.88)	0.72 (0.89)	2.71*** (0.90)		1.11 (0.75)	0.64 (0.69)	1.09 (0.70)	0.54 (0.73)	0.89 (0.74)
Period 14	-1.57** (0.78)		-1.54 (1.03)			-1.13 (0.85)	-1.62** (0.82)	-1.16 (3.44)	-1.63 (1.20)	-1.27 (0.85)
Period 15	0.63 (0.87)		-0.22 (1.14)			0.19 (1.00)	-0.30 (3.17)	0.18 (0.96)	-0.28 (0.97)	0.10 (1.09)
Period 17	-0.00 (0.94)									
Period 18										

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

Column (1): Coefficient estimates from Table 2.4, column (1).

Column (2): Coefficient estimates from Table 2.4, column (2).

Column (3): Coefficient estimates from Table 2.4, column (3).

Column (4): Coefficient estimates from Table 2.4, column (4).

Column (5): Coefficient estimates from Table 2.5.

Column (6): Coefficient estimates from Table 2.6, column (1).

Column (7): Coefficient estimates from Table 2.6, column (2).

Column (8): Coefficient estimates from Table 2.6, column (3).

Column (9): Coefficient estimates from Table 2.6, column (4).

Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
Education dummies										
Highest grade attended 6-8	3.46									
	(4.44)									
Highest grade attended 9-12	3.26									
	(4.40)									
Highest grade attended 13 or more	2.84									
	(4.41)									
Ethnicity dummies										
White	-0.16	0.05	-0.44***		-0.43***	-0.46***	-0.44***	-0.46***	-0.50**	-0.49***
	(0.11)	(0.13)	(0.14)		(0.14)	(0.14)	(0.14)	(0.14)	(0.19)	(0.13)
Age in the first year of observation										
	1.68***	1.45***	1.88***	2.26***	1.89***	1.94***	1.88***	1.92***	1.92***	1.87***
	(0.16)	(0.27)	(0.22)	(0.27)	(0.23)	(0.22)	(0.23)	(0.24)	(0.21)	(0.32)
Age in the first year of observation squared										
	-0.03***	-0.03***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Region of Residence dummies										
Mid-atlantic	0.17	0.19	0.03	-0.23	0.04	0.01	-0.13	0.08	-0.15	-0.17
	(0.58)	(2.43)	(0.64)	(0.69)	(0.65)	(0.66)	(0.71)	(0.69)	(0.89)	(0.31)
Mid-west	0.20	0.17	-0.03	0.06	-0.05	-0.02	-0.07	0.02	0.00	-0.07
	(0.40)	(2.33)	(0.54)	(0.55)	(0.52)	(0.57)	(0.58)	(0.60)	(0.33)	(0.23)
South Atlantic	0.19	0.17	-0.01	-0.26	-0.02	-0.03	-0.07	0.01	-0.06	-0.10
	(0.48)	(2.37)	(0.60)	(0.60)	(0.57)	(0.50)	(0.68)	(0.61)	(0.57)	(0.24)
East South	0.34	0.34	0.13	0.06	0.10	0.08	0.02	0.16	0.08	0.03
	(0.61)	(2.28)	(0.71)	(0.84)	(0.71)	(0.71)	(0.80)	(0.74)	(0.76)	(0.31)
West South	0.29	0.32	0.03	0.35	-0.00	-0.12	-0.04	0.10	0.06	-0.03
	(0.52)	(2.32)	(0.61)	(0.65)	(0.65)	(0.66)	(0.70)	(0.68)	(0.81)	(0.27)
Mountain	0.60	0.38	0.49	0.77	0.46	0.52	0.37	0.55	0.49	0.38
	(0.60)	(2.38)	(0.73)	(0.70)	(0.68)	(0.72)	(0.75)	(0.77)	(0.61)	(0.33)
Pacific	0.14	-0.15	0.12	0.06	0.11	-0.08	-0.13	0.17	0.14	-0.03
	(0.76)	(2.30)	(0.69)	(0.76)	(0.68)	(0.82)	(0.86)	(0.77)	(0.94)	(0.32)
State-level Economics Indicators										
Median annual income	-0.00***	-0.00*	-0.00	-0.00	-0.00	-0.00	-0.00**	-0.00	-0.00	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Top 10 percentile annual income	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female labor force participation rate	0.62	2.24	0.26	-2.92	0.36	-0.30	0.29	0.08	0.21	0.78
	(2.73)	(2.74)	(2.99)	(3.80)	(2.90)	(3.01)	(3.19)	(2.96)	(3.10)	(1.67)
Female unemployment rate	3.82	3.87	5.77**	11.33***	6.01**	6.21**	6.14***	5.53**	5.84*	6.19**
	(2.47)	(3.39)	(2.50)	(3.46)	(2.56)	(2.48)	(2.34)	(2.35)	(3.07)	(2.73)

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

Column (1): Coefficient estimates from Table 2.4, column (1).

Column (2): Coefficient estimates from Table 2.4, column (2).

Column (3): Coefficient estimates from Table 2.4, column (3).

Column (4): Coefficient estimates from Table 2.4, column (4).

Column (5): Coefficient estimates from Table 2.5.

Column (6): Coefficient estimates from Table 2.6, column (1).

Column (7): Coefficient estimates from Table 2.6, column (2)

Column (8): Coefficient estimates from Table 2.6, column (3).

Column (9): Coefficient estimates from Table 2.6, column (4).

Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
<i>Starting year dummies</i>										
start81	0.49*** (0.15)	0.57*** (0.19)	0.42** (0.16)	0.03 (0.24)	0.44*** (0.14)	0.43*** (0.16)	0.45*** (0.15)	0.45*** (0.15)	0.63* (0.33)	0.49** (0.22)
start82	0.68*** (0.17)	0.64*** (0.21)	0.65*** (0.19)	0.89*** (0.29)	0.62*** (0.17)	0.71*** (0.15)	0.65*** (0.16)	0.72*** (0.17)	0.37 (0.32)	0.68** (0.27)
start83	0.65*** (0.24)	0.57** (0.25)	0.61*** (0.23)	0.80** (0.36)	0.57*** (0.18)	0.67*** (0.20)	0.59*** (0.19)	0.65*** (0.16)	0.30 (0.30)	0.59** (0.30)
start84	0.72*** (0.26)	0.85*** (0.28)	0.61** (0.26)	0.40 (0.41)	0.55** (0.24)	0.69*** (0.23)	0.59** (0.24)	0.69*** (0.24)	0.33 (0.30)	0.56* (0.30)
start85	0.39 (0.30)	0.17 (0.34)	0.67* (0.37)	0.62 (0.39)	0.56** (0.26)	0.73*** (0.28)	0.66** (0.28)	0.72*** (0.25)	-0.52 (0.40)	0.80*** (0.29)
start86	0.66* (0.35)	0.25 (0.39)	1.15*** (0.38)	1.58*** (0.48)	1.04*** (0.24)	1.28*** (0.28)	1.17*** (0.28)	1.27*** (0.27)	-0.22 (0.35)	1.09*** (0.32)
start87	0.49 (0.41)	0.52 (0.47)	0.31 (0.49)	0.19 (0.53)	0.24 (0.34)	0.38 (0.37)	0.33 (0.35)	0.41 (0.35)	0.34 (0.38)	0.40 (0.31)
start88	0.65 (0.48)	0.65 (0.47)	0.65 (0.54)	0.17 (0.51)	0.57 (0.37)	0.77* (0.43)	0.68 (0.44)	0.80* (0.41)	-0.00 (0.42)	0.63* (0.33)
start89	0.69 (0.48)	0.90 (0.57)	0.49 (0.60)	0.54 (0.58)	0.47 (0.36)	0.61 (0.44)	0.52 (0.43)	0.62 (0.40)	0.12 (0.47)	0.56* (0.31)
start90	1.38** (0.60)	1.40** (0.62)	0.96 (0.66)	1.16** (0.58)	0.95** (0.44)	1.13** (0.49)	0.95* (0.50)	1.16** (0.48)	0.09 (0.52)	0.98*** (0.34)
start91	0.83 (0.58)	1.18* (0.61)	0.58 (0.66)	0.25 (0.68)	0.63 (0.44)	0.71 (0.49)	0.59 (0.45)	0.70 (0.46)	-0.25 (0.57)	0.55 (0.35)
start92	1.15* (0.64)	1.03 (0.65)	1.00 (0.74)	1.15 (0.73)	1.04** (0.44)	1.19** (0.54)	1.03** (0.52)	1.24** (0.48)	-0.31 (0.60)	1.07*** (0.38)
start93	0.88 (0.71)	1.42** (0.72)	0.59 (0.81)	0.28 (0.78)	0.69 (0.48)	0.75 (0.58)	0.62 (0.58)	0.79 (0.52)	-0.12 (0.64)	0.61* (0.34)
start94	0.52 (0.81)	0.90 (0.79)	-0.05 (0.87)	-0.09 (0.79)	0.09 (0.55)	0.08 (0.62)	-0.01 (0.62)	0.12 (0.56)	-0.70 (0.72)	-0.03 (0.39)
start95	0.85 (0.80)	1.18 (0.78)	0.51 (0.99)	0.37 (0.94)	0.63 (0.59)	0.70 (0.74)	0.56 (0.74)	0.75 (0.67)	-0.16 (0.75)	0.55 (0.42)
start96	0.52 (0.86)	0.53 (0.85)	0.27 (1.03)	0.45 (0.93)	0.42 (0.57)	0.44 (0.75)	0.33 (0.69)	0.43 (0.67)		0.26 (0.48)
start97	0.22 (0.95)	0.59 (0.87)	-0.37 (1.08)	-1.23 (3.58)						

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

Column (1): Coefficient estimates from Table 2.4, column (1).

Column (2): Coefficient estimates from Table 2.4, column (2).

Column (3): Coefficient estimates from Table 2.4, column (3).

Column (4): Coefficient estimates from Table 2.4, column (4).

Column (5): Coefficient estimates from Table 2.5.

Column (6): Coefficient estimates from Table 2.6, column (1).

Column (7): Coefficient estimates from Table 2.6, column (2).

Column (8): Coefficient estimates from Table 2.6, column (3).

Column (9): Coefficient estimates from Table 2.6, column (4).

Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
<i>Year fixed effects</i>										
year1981	-0.10 (0.13)	-0.03 (0.18)	-0.31* (0.17)	-0.13 (0.18)	-0.30* (0.18)	-0.30** (0.14)	-0.30* (0.16)	-0.30* (0.17)	-0.30 (0.24)	-0.31* (0.17)
year1982	-0.16 (0.16)	-0.10 (0.18)	-0.40* (0.21)	-0.81*** (0.24)	-0.37* (0.22)	-0.41** (0.19)	-0.39** (0.19)	-0.39* (0.21)	-0.45* (0.26)	-0.45*** (0.14)
year1983	-0.01 (0.16)	0.16 (0.18)	-0.37* (0.21)	-0.74*** (0.24)	-0.37* (0.20)	-0.39** (0.19)	-0.37* (0.19)	-0.37* (0.21)	-0.36 (0.27)	-0.36* (0.21)
year1984	-0.10 (0.16)	0.16 (0.18)	-0.56** (0.25)	-0.89*** (0.24)	-0.54** (0.24)	-0.57*** (0.21)	-0.54** (0.23)	-0.57** (0.26)	-0.50* (0.27)	-0.52** (0.21)
year1985	-0.04 (0.18)	0.26 (0.25)	-0.54*** (0.21)	-1.03*** (0.32)	-0.52** (0.23)	-0.56*** (0.20)	-0.52*** (0.20)	-0.56*** (0.20)	-0.45 (0.29)	-0.47* (0.25)
year1986	0.28* (0.16)	0.55*** (0.16)	-0.39 (0.25)	-0.72* (0.40)	-0.19 (0.27)	-0.41* (0.22)	-0.36 (0.25)	-0.41* (0.24)	-0.60** (0.28)	-0.63 (0.41)
year1987	0.35*** (0.12)	0.63*** (0.15)	-0.24 (0.16)	-0.42** (0.17)	-0.11 (0.16)	-0.26* (0.14)	-0.22 (0.17)	-0.22 (0.17)	-0.22 (0.26)	-0.26 (0.26)
year1988	0.46*** (0.08)	0.68*** (0.15)	0.07 (0.15)	-0.29 (0.28)	0.23 (0.16)	0.05 (0.16)	0.10 (0.16)	0.03 (0.15)	0.10 (0.24)	0.06 (0.29)
year1989	0.48*** (0.07)	0.58*** (0.21)	0.24* (0.13)	0.04 (0.14)	0.40*** (0.14)	0.23* (0.13)	0.23* (0.13)	0.18 (0.14)	0.21 (0.23)	0.18 (0.21)
year1991	0.32*** (0.09)	0.37** (0.17)	0.15 (0.16)	-0.06 (0.19)	0.21 (0.17)	0.14 (0.16)	0.14 (0.16)	0.10 (0.17)	0.10 (0.23)	0.06 (0.24)
year1992	0.35*** (0.12)	0.32** (0.14)	0.01 (0.19)	-0.28* (0.17)	0.11 (0.20)	-0.01 (0.20)	0.02 (0.19)	-0.06 (0.19)	-0.07 (0.24)	-0.12 (0.23)
year1993	0.18* (0.10)	0.11 (0.20)	0.08 (0.14)	-0.38* (0.20)	0.23* (0.14)	0.06 (0.14)	0.10 (0.14)	0.03 (0.15)	0.07 (0.22)	0.03 (0.19)
year1994	0.39*** (0.10)	0.29 (0.21)	0.34*** (0.11)	0.22* (0.11)	0.37*** (0.12)	0.33*** (0.11)	0.34*** (0.11)	0.32*** (0.11)	0.31 (0.20)	0.29* (0.15)
year1995	0.01 (0.10)	0.25 (0.21)	-0.40*** (0.15)	-0.51*** (0.15)	-0.38** (0.16)	-0.39** (0.15)	-0.40*** (0.16)	-0.41*** (0.16)	-0.47* (0.25)	-0.48** (0.19)
year1996	0.19* (0.11)	0.11 (0.23)	0.21 (0.15)	-0.03 (0.16)	0.22 (0.16)	0.21 (0.13)	0.20 (0.15)	0.20 (0.15)	0.13 (0.21)	0.13 (0.25)
Constant	-26.56*** (4.59)	-20.81*** (3.69)	-25.86*** (2.78)	-30.67*** (4.01)	-25.91*** (3.24)	-26.35*** (2.94)	-25.77*** (3.05)	-26.25*** (3.21)	-25.84*** (0.93)	-26.15*** (3.60)
LR test of gam	12.40***	1.60	2.276*	15.30***	1.713*	3.60**	1.53	3.55**	1.34	0.59
Observations	10829	4794	4925	4925	4925	4925	4925	4925	4635	4635

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

Column (1): Coefficient estimates from Table 2.4, column (1).

Column (2): Coefficient estimates from Table 2.4, column (2).

Column (3): Coefficient estimates from Table 2.4, column (3).

Column (4): Coefficient estimates from Table 2.4, column (4).

Column (5): Coefficient estimates from Table 2.5.

Column (6): Coefficient estimates from Table 2.6, column (1).

Column (7): Coefficient estimates from Table 2.6, column (2).

Column (8): Coefficient estimates from Table 2.6, column (3).

Column (9): Coefficient estimates from Table 2.6, column (4).

Column (10): Coefficient estimates from Table 2.6, column (5).

Table 10: All estimates (Tables 7 and 8)

VARIABLES	(1)	(2)	(3)	(4)	(5)
	hazard	hazard	hazard	hazard	hazard
<i>Period</i>					
Period 2	-0.02 (0.17)	-0.05 (0.17)	-0.04 (0.17)	0.09 (0.16)	
Period 3	0.43* (0.23)	0.37 (0.23)	0.40* (0.23)	0.66*** (0.21)	
Period 4	0.43 (0.32)	0.34 (0.32)	0.38 (0.32)	0.77*** (0.28)	
Period 5	0.18 (0.41)	0.06 (0.41)	0.12 (0.41)	0.62* (0.35)	
Period 6	0.52 (0.48)	0.38 (0.47)	0.45 (0.48)	1.07*** (0.40)	
Period 7	0.44 (0.56)	0.27 (0.56)	0.35 (0.57)	1.08** (0.48)	
Period 8	0.35 (0.65)	0.15 (0.64)	0.24 (0.65)	1.08** (0.55)	
Period 9	0.00 (0.76)	-0.21 (0.75)	-0.12 (0.76)	0.86 (0.64)	
Period 10	0.40 (0.82)	0.16 (0.81)	0.26 (0.82)	1.34** (0.68)	
Period 11	-1.93 (1.31)	-2.19* (1.31)	-2.10 (1.32)	-0.89 (1.21)	
Period 12	-0.67 (1.07)	-0.94 (1.06)	-0.84 (1.08)	0.45 (0.93)	
Period 13	-1.64 (1.38)	-1.92 (1.37)	-1.83 (1.39)	-0.45 (1.27)	
Period 14	-1.52 (1.41)	-1.81 (1.40)	-1.71 (1.42)	-0.28 (1.29)	
Period 15	-1.35 (1.44)	-1.67 (1.43)	-1.55 (1.44)	-0.06 (1.31)	

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).

Column (2): Coefficient estimates from Table 2.7 ,column (2).

Column (3): Coefficient estimates from Table 2.7 ,column (3).

Column (4): Coefficient estimates from Table 2.7, column (4).

Column (5): Coefficient estimates from Table 2.8. 56

VARIABLES	(1)	(2)	(3)	(4)	(5)
	hazard	hazard	hazard	hazard	hazard
<i>Ethnicity dummies</i>					
White	-0.39** (0.17)	-0.37** (0.17)	-0.38** (0.17)	-0.53*** (0.18)	-0.99*** (0.261)
<i>Age in the first year of observation</i>	2.41*** (0.48)	2.32*** (0.48)	2.37*** (0.48)	2.68*** (0.48)	
<i>Age in the first year of observation squared</i>	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	
<i>Region of Residence dummies</i>					
Mid-atlantic	0.10 (0.34)	0.12 (0.33)	-0.05 (0.38)	0.09 (0.38)	-0.19 (0.935)
Mid-west	0.20 (0.31)	0.18 (0.29)	0.14 (0.31)	0.25 (0.34)	0.28 (0.822)
South Atlantic	-0.16 (0.32)	-0.17 (0.31)	-0.24 (0.33)	-0.15 (0.36)	0.32 (0.874)
East South	0.54 (0.41)	0.56 (0.39)	0.46 (0.41)	0.55 (0.45)	0.90 (1.027)
West South	0.63* (0.35)	0.74** (0.35)	0.60* (0.35)	0.69* (0.39)	1.11 (0.996)
Mountain	0.76* (0.42)	0.74* (0.41)	0.67 (0.43)	0.87* (0.46)	0.60 (0.873)
Pacific	0.25 (0.32)	0.42 (0.35)	0.04 (0.40)	0.37 (0.36)	0.55 (1.019)
<i>State-level Economics Indicators</i>					
Median annual income	-0.00 (0.00)	-0.00* (0.00)	-0.00 (0.00)	-0.00* (0.00)	-0.00 (0.000)
Top 10 percentile annual income	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.000)
Female labor force participation rate	2.24 (2.10)	2.80 (2.10)	2.25 (2.09)	2.61 (2.29)	0.03 (2.211)
Female unemployment rate	1.35 (2.65)	1.06 (2.63)	1.54 (2.65)	0.84 (2.75)	4.51 (3.413)

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).

Column (2): Coefficient estimates from Table 2.7 ,column (2).

Column (3): Coefficient estimates from Table 2.7 ,column (3).

Column (4): Coefficient estimates from Table 2.7, column (4).

Column (5): Coefficient estimates from Table 2.8.

VARIABLES	(1)	(2)	(3)	(4)		(5)
	hazard	hazard	hazard	hazard		hazard
<i>Starting year dummies</i>						
start71	0.60*	0.59*	0.59*	0.65*	start81	-0.56**
	(0.32)	(0.30)	(0.31)	(0.37)		(0.252)
start72	0.32	0.31	0.31	0.37	start82	-0.16
	(0.30)	(0.29)	(0.30)	(0.35)		(0.353)
start73	0.24	0.23	0.23	0.25	start83	-0.31
	(0.28)	(0.27)	(0.28)	(0.33)		(0.411)
start74	0.22	0.21	0.20	0.27	start84	-0.47
	(0.29)	(0.28)	(0.29)	(0.33)		(0.317)
start75	-0.67*	-0.67*	-0.68*	-0.63	start85	-0.72*
	(0.39)	(0.38)	(0.38)	(0.43)		(0.426)
start76	-0.45	-0.48	-0.47	-0.32	start86	0.09
	(0.36)	(0.35)	(0.36)	(0.38)		(0.380)
start77	-0.01	-0.04	-0.05	0.21	start87	-0.90**
	(0.40)	(0.39)	(0.40)	(0.41)		(0.389)
start78	-0.43	-0.49	-0.48	-0.13	start88	-1.04**
	(0.45)	(0.44)	(0.45)	(0.44)		(0.465)
start79	-0.33	-0.41	-0.40	0.02	start89	-0.50
	(0.50)	(0.49)	(0.50)	(0.48)		(0.475)
start80	-0.34	-0.43	-0.41	0.03	start90	-0.87*
	(0.54)	(0.53)	(0.54)	(0.51)		(0.476)
start81	-0.75	-0.79	-0.83	-0.39	start91	-0.15
	(0.59)	(0.58)	(0.59)	(0.57)		(0.536)
start82	-0.76	-0.82	-0.85	-0.35	start92_94	-0.24
	(0.62)	(0.61)	(0.62)	(0.59)		(0.590)
start83	-0.57	-0.65	-0.67	-0.12	start95_97	-0.46
	(0.67)	(0.65)	(0.67)	(0.63)		(0.709)
start84	-1.23*	-1.30*	-1.34*	-0.73		
	(0.74)	(0.72)	(0.74)	(0.69)		
start85	-0.67	-0.78	-0.78	-0.15		
	(0.77)	(0.75)	(0.77)	(0.72)		

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

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VARIABLES	(1)	(2)	(3)	(4)	(5)
	hazard	hazard	hazard	hazard	hazard
<i>Year fixed effects</i>					
year1981					-0.01 (0.344)
year1982					0.29 (0.358)
year1983					-0.10 (0.472)
year1984					-0.17 (0.416)
year1985					-0.05 (0.423)
year1986					0.11 (0.371)
year1987					0.04 (0.260)
year1988					0.30 (0.309)
year1989					0.49* (0.251)
year1990					0.39 (0.280)
year1991					0.16 (0.231)
year1992					0.30 (0.185)
year1993					0.54*** (0.185)
year1994					-0.18 (0.166)
year1995					0.48*** (0.155)
Constant	-32.21*** (5.87)	-31.30*** (5.90)	-31.69*** (5.89)	-35.38*** (5.95)	-3.17* (1.703)
LR test of gamma	47.01***	46.18***	46.51***	55.68***	10.82***
Observations	4257	4257	4257	4257	4125

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

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